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Agricultural Research Institute

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PHILOSOPHICAL TRANSACTIONS,

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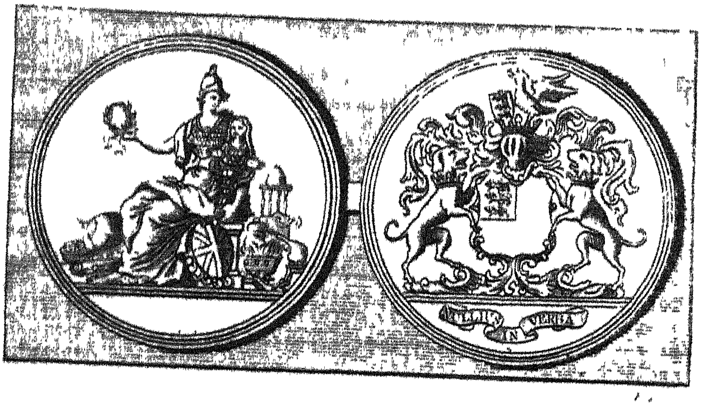
PART I.



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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the Chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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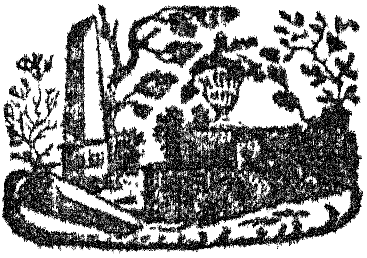


ERRATA.

VOL. LXXX.

P. 598. l. 14. *for* 307366.8 *read* 303766.8
610. l. 20. *for* p. 224 *read* p. 244.

Alſo in Gen. Roy's Table of *General Reſults*, p. 232. longitude of Wrotham Hill, *for* 19.12 *read* 18.48.

PHILOSOPHICAL

TRANSACTIONS.

I. *A Second Paper on Hygrometry.*

By J. A. De Luc, *Eſq. F. R. S.*

Read December 9, 1790.

N a Paper which I had the honour to preſent to the Royal Society in the year 1773, I ſketched the following propoſitions, as fundamental for the conſtruction of an *hygrometer.* 1ſt, That *fire,* conſidered as the cauſe of *heat,* was the only agent by which *abſolute dryneſs* could be *immediately* produced (§ 5.). 2d, That *water,* in its liquid ſtate, was the only ſure *immediate* means of producing *extreme moiſture* in hygroſcopic bodies (§ 8. and *ſeq.*). 3d, That there was no reaſon, *à priori,* to expect, from any hygroſcopic ſubſtance, that the meaſurable effects produced in it by *moiſture* were proportional to the intenſities of that cauſe; and, conſequently, that a true *hygrometrical* ſcale was to be a particular object of inquiry (§ 2.). 4th, laſtly, That perhaps the comparative changes, of the *dimenſions* of a ſubſtance, and of the *weight* of the ſame or other ſubſtance, by the ſame variations of *moiſture*, might lead to ſome diſcovery in that reſpect (§ 72.). The ſame propoſitions will be the ſubject of this Paper.

Of abſolute dryneſs.

1. An *hygroſcopic* body, which is not brought into contact with any other body *drier* than itſelf, cannot loſe any part of its *moiſture* but by *evaporation*; and if this is intirely produced by *fire*, there may be ſuch a degree of *heat* as will cauſe the total *evaporation* of that *moiſture*. This is the principle on which the above firſt propoſition was founded; but at the ſame time I mentioned, that I had not brought it into practice, becauſe of the impoſſibility of ſubmitting the ſubſtance of the hygrometer to ſuch a degree of heat. However, I ſoon removed that difficulty by conſidering, that the degree of *heat* neceſſary to produce *extreme dryneſs*, might be applied to ſome ſubſtance that could bear it; and that *dryneſs* be tranſmitted to the *hygrometer*, by incloſing it with that ſubſtance in a proper veſſel. The ſubſtance I choſe was *potaſh*; and I prepared, for this and ſome other hygrometrical purpoſes, an apparatus which was made by Meſſ. Nairne and Blunt in 1776. But a new objection ſtopped me again in that purſuit, and led me for ſome time to a very great and now almoſt uſeleſs labour. The degree of dryneſs produced by *potaſh* ſo uſed, could be only proportionable to the degree of *heat* that it had received; and

not conceiving yet any known limit to the intenſity of *heat*, I could not expect any limit to *dryneſs*, nor even a fixed degree of it.

2. I remained at that point, with however a *comparable* hygrometer laboriouſly conſtructed, till I came to conceive, that *heat* muſt be at its *maximum* in a body, when it is *incandeſcent*; which opinion I have explained in my work, *Idées ſur la Météorologie*. From that firſt idea I ſoon after concluded; that every *hygroſcopic* ſubſtance, which could retain that property after having been brought into *incandeſcence*, would anſwer my firſt purpoſe. The following is the theory reſulting from the whole of the above conſiderations. 1ſt, The *hygroſcopic* ſubſtance which has the moſt capacity for *moiſture*, and receives it the moſt readily, being placed in any quantity in a given ſpace, cannot bring that ſpace to a degree of *dryneſs* greater than its own; and if that degree is undetermined, it cannot afford any *fixed point* for the *hygrometer*. 2d, The *hygroſcopic* ſubſtance which has the ſmalleſt capacity for *moiſture*, and is the floweſt in receiving it, if it is really reduced to *extreme dryneſs*, will have the power of producing it in a given ſpace, provided its ſmall capacity be compenſated by a greater quantity, and its ſlowneſs by more time. 3d, Every *hygroſcopic* ſubſtance, which may be brought to *white heat* without loſing its property, is fit to produce *extreme dryneſs* in a cloſe ſpace. 4th, It is indifferent for that purpoſe, that the ſubſtance uſed be of the claſs which has a chemical *affinity* with *water*, it being ſufficient that, after having been reduced to *extreme dryneſs*, it be ſtill capable of receiving it from the ambient *medium*, as may every porous ſubſtance. 5th, But for the practical purpoſe of fixing the point of *extreme dryneſs* on *hygrometers*, ſuch a ſubſtance muſt be choſen as, with a great

capacity for *moiſture*, receives it but ſlowly: as by that firſt property it may be taken in leſs quantity; and by the laſt, it will be leſs ſubject to acquire a ſenſible quantity of *moiſture* in the time neceſſary for the operations.

3. *Pot-aſh* and ſome other alkaline ſubſtances afforded the firſt of thoſe properties, but not the laſt; and I had not fixed on any ſubſtance, when, being at Birmingham in the autumn of 1782, Mr. JAMES WATT informed me, that his friend Dr. BLACK had found in *quicklime* a great capacity for *moiſture*, and much ſlowneſs in retaking it: this he knew, by having kept a long time the ſame *lime* in a cloſe veſſel, for drying ſalts and capillary tubes for thermometers. Theſe were the very properties I wanted for my purpoſe, which thereby I executed as ſoon as I came home. I made thoſe firſt operations in ſmall glaſs veſſels, uſing old *lime*, which I brought again to *white heat* every time I uſed it. Theſe firſt trials agreed with my theory in its firſt point; that of producing conſtantly the ſame degree of *dryneſs*: as for the ſecond, namely, whether that degree was *extreme*, it depended on other experiments.

4. Being ſure then of a fixed degree of dryneſs, the number of experiments I undertook made me wiſh for a means of avoiding the frequent repetitions of bringing again my *lime* to *white heat*; and having found one which has ſucceeded, I am going to deſcribe the apparatus. The veſſel, fig. 1. is of tin, 3 feet high and 1 in diameter. A glaſs plate *a*, *a*, *a*, *a*, is fixed at the top, forming a vertical ſection of the cylinder at 1 inch diſtance forwards from the axis. A woven braſs-wire cage *b*, *b*, *b*, *b*, is fixed in the veſſel through its diameter, in order to keep a ſpace for the inſtruments; for the ſame purpoſe it is open at the top, and alſo oppoſite the glaſs, where the dials of the inſtruments are to be ſeen. For my experiments.

ments, which required inſtruments of various ſizes, I made that cage 18 inches high and 2 deep; but it may be much ſmaller for common hygrometers. The whole veſſel, except that ſpace, has been filled, through the openings *c*, *c*, with *quicklime* taken from the kiln, and ſuffered only to loſe the red heat; after which the openings were covered with heaps of the ſame lime, which abſorbed the moiſture of the air entering the veſſel while it was cooling, and then the openings were ſhut with tin plates and putty. The top of the veſſel has four ſquare openings *d*, *d*, *d*, *d*, correſpondent to the wire cage, for the introduction of the inſtruments, which are hung to hooks. I uſe a hooked wire for putting in or taking out the inſtruments, to avoid bringing my fingers near the openings. Theſe are kept ſhut with tin plates and putty: I never open but one at a time, which I leave open as little as poſſible; and to prevent the introduction of the external air in thoſe ſhort operations, I make them as nearly as poſſible at the ſame temperature, which being 60° of FAHRENHEIT may be obtained in every ſeaſon. With theſe precautions, and alſo by moiſt air being lighter than dry air, there is ſcarcely any moiſture introduced in the veſſel except by the inſtruments themſelves.

5. This application of my method afforded me a very ſtrong confirmation of the practical fixity of the point of *dryneſs* produced in that manner by *lime*: for the apparatus was different from the former ones; 1ſt, by the quantity of the *lime*; 2d, by the *lime* having been put very hot into the veſſel, while, when I uſed glaſs veſſels, I had ſuffered it ſometimes to cool down to 60°; 3d, by that *lime* being of the firſt calcination, inſtead of old *lime* brought again to white heat; and all theſe differences produced no ſenſible effect on the point of *dryneſs*. Since that time Meſſ. NAIRNE and BLUNT, Mr. HURTER, and Mr.

HAAS, have made apparatuſes of the ſame kind, and I have made myſelf ſome others of different ſizes and ſhapes; and they all produce ſenſibly the ſame degree of *dryneſs*.

6. The deſcribed apparatus was ready in the month of October, 1787, and I put in it one of my firſt *hygrometers*; which, in a few days, came to its fixed point of *dryneſs*, and there it has remained ever ſince, though I have opened the veſſel above four hundred times. That degree of conſtancy, much beyond my expectation, has enabled me to make a variety of experiments, which elſe had been next to impoſſible: it proceeds partly from the great capacity of *quicklime* for *moiſture*, which I ſhall determine hereafter; and partly from its flowneſs in receiving it; which circumſtances, added to the ſmall ſize of the openings, to their being at the top of the veſſel, and to the care of putting in and taking out the inſtruments nearly at the ſame temperature, prevents the *lime* from acquiring any ſenſible degree of *moiſture* during theſe operations.

7. I did not truſt at firſt the apparent continuance of the ſame degree *of dryneſs* in that veſſel. At the end of nine months of frequent uſe, I began to fear, leſt the *whalebone*, of which the *ſtandard hygrometer* is made, had been impaired; and I took it out, to try its point of *extreme moiſture* (of which I ſhall ſpeak hereafter;) but it came exactly to that point, and when put again into the lime-veſſel it returned where it ſtood before. I have repeated many times that trial, with the ſame reſult; the laſt time was at the end of three years, when, inſtead of a loſs of expanſibility in the *whalebone*, I found a ſmall increaſe, but probably accidental; it went a little farther than its point of *extreme moiſture*, and came back to its conſtant point of *dryneſs*.

8. The principles of hygrometry being now my only object, it would not be proper to enter into particulars on its practical part; but I ſhall here mention for once, that the ſteadieſt *hygroſcopic* ſubſtances are ſubject to anomalies: for inſtance, after an hygroſcope has remained fixed in *water* for many hours, if it is taken out, ſuffered to dry a little, and then put again into *water*, it may ſometimes happen to overpaſs that point. In the ſame manner, after an hygroſcope has been long fixed in the lime-veſſel, it may happen alſo, that in taking it out only for a quarter of an hour, and putting it in again, it will move a little farther than it was before. Again, if in taking it out of the *lime-veſſel*, where it had long remained fixed, it is put into *water*, and then back into the *lime-veſſel*, it may happen, that it will fix itſelf a little ſhort of its former point, and never move thence, except by repeated great variations of heat; but if, when it ſhews that diſpoſition, it is taken out for a ſhort time, and put in again, it will then attain its uſual point. This was the caſe in the laſt trial of my *ſtandard*. Laſtly, the ſame anomalies may take place at every other point of the ſcale of every hygroſcope, only more or leſs according to the ſubſtances; ſome of which, for that reaſon, cannot be uſed for practical hygrometry.

9. Thoſe anomalies of the ſteadieſt *hygroſcopic* ſubſtances, will probably prevent our ever having in the *hygrometer* an inſtrument nearly ſo exact as the *thermometer*; and this I was to premiſe, that when I mention the reſults of particular *hygroſcopic* experiments, it may be underſtood, that they have only the degree of exactneſs that belong to their claſs. Luckily thoſe anomalies are yet of no conſequence for the great objects of *hygrology* and *meteorology*; the preſent ſtate of *hygrometry* being ſufficient to excite on thoſe objects, queſtions of great importance for

natural philoſophy. And in the mean time thoſe anomalies are very intereſting in themſelves; as, from their laws, they ſeem to point out ſome modification of *cohéſion*, as being the immediate cauſe of *elaſticity* in *ſolids*. If I can find time to put in order a number of obſervations and experiments I have made in that reſpect, I intend to make it the ſubject of a Paper, in which I ſhall examine, from a general reſult of thoſe phænomena, the comparative uſe of *weights* and *ſprings* for keeping ſtretched the *hygroſcopic* ſubſtance in *hygrometers*.

10. After *fixity* in the degree of *dryneſs* produced by *lime* in the manner I have explained, the next point to be examined in reſpect of my theory, was, if the *nature* of the ſubſtance brought to a *white heat*, had any influence on the degree of *dryneſs* thereby produced; and in order to try at once the effect of a very great diſparity, I choſe ſuch a *ſand-ſtone* as is not affected by *acids*, and ſtrikes fire from ſteel, before and after having been *incandeſcent*. The firſt experiment I made was with a view of finding the comparative capacities for *moiſture* between that *ſtone* and *lime*. For that purpoſe I took ſuch pieces of them as might be readily reduced to half an ounce while *incandeſcent*; which being done, I put them into braſs cups, fitted to a ſcale, and I incloſed them under a glaſs veſſel inverted over *water*. I weighed thoſe ſubſtances from time to time; each of them continued to acquire *weight* during five weeks; at which time the *ſand-ſtone* had gained $\frac{1}{250}$ part of its original *weight*, and the *lime* $\frac{110}{250}$: this laſt was at that time all cracked and fallen in ſmall fragments, eaſily reduced to powder; the *ſand-ſtone* ſtruck fire as before. I next prepared a cylindrical tin veſſel, 10 inches in diameter and 14 inches high, with a glaſs top, which I filled with fragments of that *ſtone*, treated as the *lime*; and when it was cooled, I

put into it an *hygrometer*, whoſe fixed point of *dryneſs* has been taken in the *ſame* apparatus: and in five weeks it was fixed to the ſame point. This is a demonſtration, that the *nature* of the ſubſtance does not interfere with the *degree* of *dryneſs* produced, and that *incandeſcence* is the only cauſe of its *fixity*.

11. Laſtly, in reſpect of *hygrometry*, a degree of *dryneſs* thus determined might have been ſufficient; but for *hygrology*, and even for natural philoſophy in general, it was deſirable to diſcover if that *fixed degree* of *dryneſs* was alſo *abſolute*: and the following are the conſiderations which directed me in that enquiry: if *evaporation* is produced by *heat* only, and if *incandeſcence* is the *maximum* of *heat*; an *hygroſcopic* body, which is brought to *incandeſcence*, cannot contain any *evaporable* water; and if that body has ſuch a maſs, as to be capable of abſorbing all the water *evaporated* in a certain ſpace, without acquiring any meaſurable *moiſture*, that ſpace may be called *abſolutely dry*. Now, if an *hygroſcopic* ſubſtance which is incloſed in that ſpace, contains any ſenſible quantity of *evaporable* water, when *heat* increaſes, that ſubſtance muſt loſe a part of its *moiſture* in the *medium*, and take it back by the diminution of heat. Conſequently there was a means of diſcovering, if *hygroſcopic* ſubſtances, reduced to the above degree of *dryneſs*, ſtill retain a ſenſible quantity of *evaporable* water; it was that of obſerving their *weight*, by changes of heat: and from theſe previous conſiderations I made the following experiments.

12. I hung ſucceſſively to a very ſenſible *beam*, ſhewing the changes of *weight* by an *index*, different ſorts of vegetable and animal ſubſtances, the *beam* being incloſed in a glazed tin-veſſel, containing a ſufficient quantity of *quicklime*; and during the operations, I produced from time to time great changes in the *temperature* of the veſſel. As long as theſe ſubſtances

retained a ſenſible quantity of *evaporable* water, the increaſe of *heat* made them loſe ſome *weight*, which they regained partly when the *heat* returned to the ſame point. But that effect diminiſhed by degrees; and, at laſt, a change of 30° of *Fahrenheit* did not produce any ſenſible change of *weight* in thoſe ſubſtances, though they were ſuch as had a great capacity for *moiſture*. An *hygrometer* placed near the *beam*, was then at the point taken in the *lime-veſſel*. That ſingle experiment confirms all the previous conſiderations from which I had expected *abſolute dryneſs* from *incandeſcence*.

Of extreme moiſture.

13. The ſecond propoſition I had ſketched in my firſt paper, is this "that *water*, in its liquid ſtate, is the *only* ſure immediate "means of producing *extreme moiſture* in *hygroſcopic* bodies."

14. *Moiſture*, the nature of which we are firſt to determine, may be conſidered in three different caſes.—1ſt, In ſubſtances which have an *affinity* with *water*; by which their moleculæ and thoſe of *water* may unite, and form a new compound.—2dly, In ſubſtances which have no *affinity* with *water*, but to which *water* has a tendency to adhere; by which cauſe it enters their *capillary pores*.—3dly, In the *medium*, or ſpace free from viſible bodies. I have not undertaken to diſcover what, in the firſt of theſe caſes, might properly be called *moiſture*, and its *degrees*; as I foreſaw great difficulties in that undertaking, which beſides was unneceſſary to my principal purſuit: therefore I come immediately to the ſecond caſe.

15. When I wrote my work *Idées ſur la Météorologie*, having not yet made ſome experiments I had in view to verify the opinion I entertained, that vegetable and animal ſubſtances, as

well as the porous mineral ones, received *water* merely by the faculty of *capillary pores*, I uſed ſometimes the expreſſion *hygroſcopic affinity*, in treating of the *hygoſcropic equilibrium*; but before that work was come out of the preſs, having converſed on that object with Dr. BLAGDEN, and found him inclined to that opinion, I had time and opportunity to expreſs it, in § 276, as follows: "There are reaſons to doubt, whether ſome "of the ſubſtances, which ſhare amongſt them the *water* diſ-"ſeminated in a ſpace, do not *ſuck* it, by a faculty ſimilar to "that of *capillary tubes*, without any *chymical affinity* with "*water*." That opinion will be now confirmed by the following experiments.

16. 1ſt. EXP. *Sugar* has an *affinity* with *water*, and no ſenſible one with *alcohol*: however, a lump of *ſugar* will *imbibe* this laſt *liquid* as readily as the firſt. Conſequently, *water* is not *imbibed* by ſugar in conſequence of their *affinity*; ſince *alcohol* is alſo *imbibed*: they both aſcend in *ſugar*, by the faculty of its *capillary pores*, as they do in *ſand-ſtone* or in *ſpunge*. But when *water* has thus entered *ſugar*, it *diſſolves* it; which then is a *chymical* effect; whereas *alcohol* evaporates, and leaves the *ſugar* ſenſibly as it was before.

17. 2d. EXP. If *water* penetrated *hygroſcopic* ſubſtances of the vegetable and animal kinds, by an *affinity* with them, it would not be natural to expect, that other *liquids*, which do not ſhew an *affinity* with the ſame ſubſtances as *water*, ſhould penetrate the above-mentioned ſubſtances. Being led by that conſideration, I made two *hygroſcopes* of different elaſtic *animal* ſubſtances; and after having marked the point where they ſtood in *water*, I immerſed them ſucceſſively in *alcohol* and in *ether*; by which *liquids* they were expanded nearly as much as by *water*, and they contracted as much in coming out of them.

In

In thoſe experiments a ſingular phænomenon happened in both hygroſcopes. The firſt effect of the immerſion of thoſe *animal* ſubſtances in *alcohol* (and I ſuppoſe it would have been the ſame with *vegetable* ones) was *contraction*, ſoon followed by *expanſion*; and when they came out into the air, the firſt effect was *expanſion*, ſoon followed by *contraction*. The cauſe of that reciprocal phænomenon is undoubtedly the *affinity* of *alcohol* with *water*. In the immerſion, ſome of the *moiſture* came out of the ſubſtance, to unite with the ſurrounding *alcohol*; by which loſs of *water*, a *contraction* took place in the ſubſtance, 'till it came to imbibe the *alcohol* itſelf. In the immerſion, ſome of the *moiſture* of the air, uniting immediately with the *alcohol* retained by the ſubſtance, expanded it to the ſame degree as if it had been in *water*; after which, the *alcohol* evaporating, the ſubſtance contracted. Thoſe phænomena did not happen with *ether*; this not uniting readily with *water*; but it expanded thoſe ſubſtances as much as *alcohol*, and nearly as much as *water*. From thoſe phænomena we may conclude, that the penetration of animal ſubſtances by *water*, and conſequently by *moiſture*, is produced, as that of *ſugar*, *ſand ſtone*, and every other porous ſubſtance, by the faculty of *capillary pores*, without any *affinity* between them and *water*,

18. 3d. Exp. In that theory, of a mere *imbibition* of *water* by *hygroſcopic* ſubſtances of the *elaſtic* kind, a circumſtance, which ſeems to point out *affinity*, was to be explained; it is that of the *hygroſcopic equilibrium*. In view of that object, I made the following experiments, not new in themſelves, but directed to my purpoſe. I took ſome glaſs tubes, of different ſmall bores, which I firſt bent in the ſhape of ſyphons; after which I cut them in the middle of the bent part. This was to enable me, to bring into exact communication the lower end of two tubes,

though held in a vertical poſition, as an inverted ſyphon (Fig. 2.) The following are the experiments.—1ſt. Exp. When a column of the liquid had aſcended in one of theſe tubes; if I applied to it an empty tube of the ſame bore, the *liquid column* divided itſelf equally between them.—2d. Exp. When the empty tube was of a ſmaller bore, that *column* roſe more in it, than it ſunk in the other; and the contrary happened when the empty tube was of a larger bore—3d. Exp. When ſome more *liquid* was ſupplied to the united tubes, it roſe in both, in proportion to the reſpective heights of the former *columns*.—4th. Exp. When a ſuperabundant quantity of *liquid* was ſupplied, it roſe to a *maximum* in each tube, and the heights of the columns increaſed in ſome proportion with their former heights.

19. Theſe known facts have a clear analogy with the *hygroſcopic equilibrium* in *elaſtic* ſubſtances.—1ſt. Case. When the quantity of *liquid* common to *capillary tubes* is not ſufficient for them to receive their reſpective *maximum*, they ſhare it between them, and the *equilibrium* takes place, when there is, in each of them, the ſame ratio between its ſpecific *capillary power* and the weight of the raiſed column. In the ſame manner; when the quantity of *water* diſſeminated in a ſpace, is not ſufficient, for ſeveral hygroſcopic ſubſtances to receive the *maximum* of *water* which they can contain in their *pores*, they ſhare it amongſt them; and the *equilibrium* is produced, when there is in each of them the ſame *ratio*, between its ſpecific *capillary power*, and the reſiſtance of their *pores* to be more dilated.—2d. Case. When there is a ſuperabundant quantity of *liquid* common to ſome *capillary tubes*, each of them receives its *maximum*; which is determined by an *equilibrium*, between its total *capillary power*, and the *weight* of the raiſed *column*. In the ſame manner, when there is a ſuperabundant quantity of *water* common

to ſeveral *hygroſcopic* ſubſtances, each of them receives its *maximum*; which is determined by the *equilibrium*, between their total *capillary power*, and the reſiſtance of their *pores* to be more dilated. That final *equilibrium*, which, from its very nature, cannot be overpaſſed in any *elaſtic* ſubſtance properly uſed, determines the ſpecific *capacity* of thoſe ſubſtances for *moiſture*.

20. *Moiſture* then, conſidered in *porous* bodies not *ſoluble* by *water*, may be defined, "A quantity of *water*, which is "*inviſibly* contained in their *pores*; without any other connec-"tion with their ſubſtance, than that which it has with the "*glaſs* of the *capillary tubes* into which it has aſcended."

21. We may ſee now whence proceeds the *hygroſcopic equilibrium* between *elaſtic* ſubſtances incloſed in a ſpace, either filled with *air* or deprived of it. In this explanation it is unneceſſary to determine, how *water* is *inviſibly* diſſeminated in ſpaces free from viſible bodies, therefore I ſhall not enter here into this ſubject; that diſſemination is a fact admitted in every hypotheſis, conſequently the *medium* is only to be conſidered as the ſtock and ſtandard of *moiſture*. By the cauſe, whatever, of *evaporation*, *hygroſcopic* ſubſtances loſe or gain *water* in the *medium*, according to its degree of *moiſture*, till they are in *equilibrium* with it; which implies the *equilibrium* amongſt themſelves according to the laws reſulting from their own nature.

22. Now *moiſture*, in a general ſenſe, will appear to be, "a "quantity of *inviſible water*, either *evaporable*, or *evaporated*." And from that definition, the *maximum* of *moiſture* will exiſt, when, "every circumſtance remaining the ſame, no more "*water* can be admitted in a ſpace, without becoming *viſible*; "on *ſolid* bodies, by their ſurface being *wet*; and in the *medium*,

"by a ſpontaneous *precipitation* of *water*." Laſtly, as immerſing ſolid *hygroſcopic* bodies in *water*, or expoſing them in a *medium* where there is an actual *precipitation* of *water* (as in a *fog*) is an effectual means of furniſhing their *pores* with the whole quantity of *water* they can *imbibe*; it is evidently a *ſure* means of producing *extreme moiſture* in them: and this point cannot be overpaſſed, neither in *water*, nor in *fog*, ſince it depends upon the *reſiſtance* of the *pores* to further dilatation by the mere introduction of *water*; but it muſt be attained in an *hygrometer*, to fix its point of *extreme moiſture*.

23. When formerly I had fixed upon that method for procuring to my firſt *hygrometer* a true point of *extreme moiſture*, it occurred to me, that the *temperature* of *water* might influence ſenſibly the expanſion of its *ivory* tube; and in order to diſcover if it was ſo, I made ſome experiments, related in §§ 104 &c. of my former paper, the reſult of which was, that the *temperature* of *water* had a ſenſible effect on the expanſion of *ivory*. But ſoon after I diſtruſted ſome modifications of that complicated *hygrometer*; and eſpecially this particular reſult.

24. I then changed that firſt method; which conſiſted in meaſuring the changes of capacity of hollow cylinders, into that of meaſuring the changes in length of *hygroſcopic* ſubſtances; and for ſome preliminary experiments on many of them, I made particular *frames*, in which, by a combination of *glaſs* and *braſs*, the effects of *heat* on thoſe materials compenſated each other; by which means the *indices* of thoſe inſtruments were only affected by the modifications of the *hygroſcopic* ſubſtances tried in them. I have mentioned theſe *frames* in a paper on *pyrometry*, printed in the *Phil. Tranſ.* for 1778. With theſe *frames* I firſt tried *ivory*, in *water* of different *temperatures*;

and I found a very little difference in its *expanſion*, comparatively with what had appeared from my firſt experiment. Then, continuing the ſame trial on various ſubſtances, I found the effect of different *temperatures* of *water* very ſmall in general; and even in ſome ſubſtances, as *deal* taken lengthwiſe, and *hemp*, I could not aſcertain any.

25. Theſe experiments led me to think, that the ſmall variations produced by *heat* in hygroſcopic ſubſtances dipt in *water*, were not *hygroſcopic* modifications, but the mere effects of *heat*, by the ceſſation of all *hygroſcopic* modifications; theſe having then attained their *maximum:* which is a diſcrimination of effects, that I had vainly attempted to produce by other means. When afterwards I had found the method of producing *extreme dryneſs*, I made a lime apparatus, for the purpoſe of repeating in it the ſame experiments with my *compound frames*; and I found that theory confirmed, by the effects of *heat* in that apparatus being nearly the ſame, on the ſame ſubſtances, as when they were in *water*.

26. From the whole of the foregoing experiments there cannot remain any doubt, that *water*, in its liquid ſtate, is a *ſure* means of fixing the point of *extreme moiſture* on *hygrometers*. Particularly, in reſpect of *elaſtic* ſubſtances, as *ivory*, *quill*, *whalebone*, all ſorts of *wood*, and a number of others which I have tried, the laſt experiments in *water* of different *temperatures*, afford an immediate proof, that their faculty of *ſucking* water has a fixed limit, proceeding from a final *reſiſtance* of their *pores*, to be more dilated by the introduction of *water*. Conſequently, their *utmoſt expanſion* is a true ſign, that *moiſture* is *extreme* in them; which point cannot be exceeded. But my propoſition extended farther: I had ſaid, that *water* was the *only* certain means of obtaining immediately the point of

extreme *moiſture* on *hygrometers*; and this is a moſt important queſtion, both of *hygrometry* and *hygrology*, which remains to be examined.

On the maximum of evaporation, *and its correſpondence with the* maximum of moiſture *in a* medium.

27. Since *moiſture* conſiſts in *inviſible* water, an *exceſs* of *water* is the only immediate means of aſcertaining that the *maximum* exiſts; as, if a reſervoir is above our reach, the only means of knowing if it be *full* of water, is when it overflows. From that principle, a *fog* gives the point of *extreme moiſture* on *hygrometers*, like *water* itſelf; becauſe it covers very ſoon the *hygroſcopic* ſubſtance with a coating of *water*: ſometimes even it expands it a little more than an immediate application of *water*; but this belongs to an object that I have waved before, as relating to ſome modifications of *elaſtic ſolids* (§ 8.). No other means then but an *exceſs* of *water* over the ſurface of the *hygroſcopic* ſubſtance of the *hygrometer*, can aſcertain that it is arrived at its point of *extreme moiſture*; and the firſt immediate demonſtration I ſhall give of it, will be afforded by *dew*; a very uncertain, though apparently certain ſign of *extreme moiſture* in the *air*. We ſay, that there is *dew*, when ſome ſolids expoſed in the open air in a clear evening are *wet*; but if that was the effect of a *precipitation* of *water* happening in the *air*, all the ſolids thus expoſed would be *wet*; which is far from being the caſe; conſequently, that phænomenon muſt proceed from ſome particular cauſes, by which, though no *water* is yet diſpoſed to abandon the *medium*, it gathers on ſome particular ſolids. It is very long ſince the phænomena of *dew* have perplexed natural philoſophers; and they were the firſt

which

which I ſtudied in the beginning of my reſearches in meteorology; but all that I concluded from my experiments and obſervations was, that we could not underſtand thoſe phænomena without firſt having a ſure *hygrometer*. This is the reaſon why, ſoon after I had made my firſt *hygrometer*, I expoſed it in the open air in the country, ſuſpended very little above the graſs, from the morning of a fine day to the time of *dew* in the evening; the *graſs* grew *wet*, and the *hygrometer* remained at a great diſtance from the point which had been fixed in *water*. I have related that experiment in § 91, of my firſt Paper.

28. When I had made hygroſcopes of various ſorts of *ſlips*; for inſtance, of different *woods* and of *whalebone*, cut acroſs the fibres; of *ivory* and *horn*, reduced firſt into thin tubes, and then cut in ſcrew; and of *quills*, by cutting alſo in ſcrew their barrels; I repeated, with thoſe inſtruments, my obſervations on *dew*; and to give a ſhort, but determinate idea of the phænomena I obſerved, I ſhall reduce them to ſome general caſes, as indicated by one only of thoſe *hygroſcopes*, that of *quill*, which, like all the others, is divided into 100 parts, from *extreme dryneſs* to *extreme moiſture*. Theſe hygroſcopes were ſuſpended in the open air, three feet above a graſs-plat in the country. 1ſt Case. When a clear and calm evening ſucceeds to a clear and warm day, the *graſs* frequently grows *wet*, though the above *hygroſcope* ſtands many hours, and ſometimes the whole night, between 50 and 55. 2d Case. If the *dew* increaſes, ſo that taller herbaceous *plants* and *ſhrubs* grow *wet* in ſucceſſion, the *hygroſcope* moves more and more towards *moiſture*; and when it is come to about 80, plates of *glaſs* and *oil-paint* alſo grow *wet*; but at that period, neither *metallic* plates, expoſed like the glaſs ones, nor ſome *ſhrubs* and *trees*, are *wet*; and this alſo may laſt whole nights. 3d Caſe. If the

dew proceeds to its *maximum*, the *hygroſcope* moves from 80 to 100 (and ſometimes a little farther, § 27). Then we have alſo a certain proof that *extreme moiſture* exiſts in the air; for every ſolid body expoſed to it is *wet*. But it is only at that moment that we can depend on *extreme moiſture* exiſting; for, if in the other deſcribed ſtages of the phænomenon, the appearance of *water* on the ſurface of ſome ſolids had proceeded from a ſpontaneous precipitation in the air, all the other ſolids ought to have been *wet*; but they only become *wet* in a certain ſucceſſion, and in the mean time the ſlip of *quill*, and all the other above-mentioned *hygroſcopes*, move more and more towards their point 100, in ſign of *moiſture* increaſing in the air. Conſequently (as I had concluded from my firſt obſervations), inſtead of having in *dew* an *hygroſcopic* ſtandard for the *hygrometer*, we have in its phænomena many circumſtances which will only be explained with the aſſiſtance of that inſtrument.

29. Some previous obſervations had alſo warned me againſt the general idea, that *moiſture* was to be *extreme* in the *air*, when there was a ſufficient quantity of *water* in the ſpace, even though that *air* might be ſuppoſed to be filled with *evaporated* water to its *maximum*; and the doubts I entertained in that reſpect were the cauſe of the difficulties I expreſſed in the beginning of my firſt Paper, which I only got over when I thought of *water* itſelf, to obtain the point of *extreme moiſture* on my *hygrometer*. This was alſo the reaſon why, as ſoon as my firſt *hygrometer* was finiſhed, I placed it in a cellar, the walls and ground of which were *wet*, and where it continued two months, without ever attaining its point of *extreme moiſture*. I have related that experiment in § 54. *et ſeq.* of my firſt Paper.

30. When alſo I had the *hygroſcopes* mentioned above in the obſervations on *dew*, I undertook a very long courſe of various ſorts of experiments on that important point of *hygrology*,

grology, of which however I ſhall only give here the general and conſtant reſults, as furniſhed by thoſe hygroſcopes. "The "*maximum* of *evaporation* in a maſs of incloſed *air* is far from "being identical with the *maximum* of *moiſture*; this being "dependent alſo, even to a very great degree, on the *temperature* of the *ſpace*, ſuppoſed to be the ſame, or nearly ſo, as that "of the *water* which *evaporates* in it. *Moiſture* may arrive to "its *extreme* in an incloſed *air*, if that common *temperature* "is near *freezing point*; but it becomes leſs and leſs, even to a "very *dry* ſtate, as that *temperature* riſes, though the *product* "*of evaporation*, thereby increaſing, continues to be at its dif"ferent *maxima*, correſpondent to the different *temperatures*." This is a very important propoſition in *hygrology*; which, from my experiments, would not be ſubject to any objection, if there were no other *hygroſcopes* than thoſe I have mentioned above, of which I have thirteen different ſpecies; but there is another claſs of ſuch inſtruments, from which ſome doubt might firſt ariſe; and I come now to that point.

On two diſtinct claſſes of hygroſcopes.

31. As I ſhall now frequently ſpeak of *ſlips* and *threads*, which conſtitute thoſe two *claſſes* of *hygroſcopes*, I muſt firſt explain what I mean by thoſe words. The *ſlips* compoſe the claſs of *hygroſcopes* uſed in the above experiments; they conſiſt of very thin and narrow *laminæ* cut *acroſs* the fibres of vegetable or animal ſubſtances, either in their natural or artificial breadths (as *boards*), or by reducing natural or artificial thin tubes of them into *helices*. By *threads* I mean the ſame kinds of ſubſtances taken *lengthwiſe*, either from their being naturally in thin threads, or by reducing them to that ſtate. in

earing from them thin *faſciculi* of *fibres*; which operation is eaſy in ſome, as *hemp*, *whalebone*, and *gut*, but very difficult in others, as *quill* and ſome ſorts of *wood*.

32. The firſt *hygroſcopes* of the claſs of *threads*, which I obſerved comparatively to the claſs of *ſlips*, were of *hemp*, *gut*, *whalebone*, and ſome *woods*, and they exhibited a phænomenon which at firſt I could not underſtand: when they were expoſed with the *ſlips* in *damp* air; as, for inſtance, in open air during the ſecond period of *dew* above determined, or in a glaſs veſſel inverted over *water*; the *threads* had only very ſmall motions backwards and forwards round their point determined in *water*, while the *ſlips* had conſiderable motions within that point, without coming near it, if the *temperature* was ſenſibly above *freezing point*. Thence ariſes the objection againſt the general propoſition above ſtated: for theſe two claſſes of *hygroſcopes* contradicting one another on the changes of *moiſture*, nothing could be aſſerted in that reſpect, till there were ſufficient reaſons to exclude one of thoſe claſſes of informers, and to truſt the other claſs.

33. Proceeding to multiply the ſpecies of thoſe two claſſes of *hygroſcopes*, I found always the ſame fundamental *march* in *ſlips*, all of them conſtantly moving in the ſame direction; but in multiplying the ſpecies of *threads*, I found ſuch variety between them, that in their own claſs they created diſtruſt; ſome of them, as of *deal*, *aloes pitta*, *liber of lime tree*, *quill*, and thin *ſtems of gramen*, in coming out of *water*, increaſed in *length*: they went farther that way, to a certain point, as the air was dryer: they *retrograded* then with accelerated ſteps, when *dryneſs* increaſed, thereby returning to the point where they had ſtood in *water*; and they continued to move in the ſame *retrograde* way, with great acceleration, by a ſtill in-

creaſing *dryneſs*. Moreover, they did not follow one another in thoſe motions contradictory to the evident *march* of *moiſture*: each of them changed direction at different periods, thereby often contradicting alſo each other, while the *ſlips* conſtantly agreed together in the direction of their motion, and alſo with all the other ſymptoms of *moiſture*. From theſe comparative phænomena I firſt concluded; that the motions of the ſame kind, which I had obſerved in the firſt-mentioned *threads*, were alſo *anomalies*, proceeding, only to a ſmaller degree, from a cauſe of the ſame nature as that of theſe laſt. After which, ſimilar ſymptoms, which I had formerly obſerved in the *water* thermometer near the *freezing point*, made me firſt conclude, from a general analogy; that the perceivable modifications of the *threads*, were the compound effects of two contrary operations of *moiſture* which followed different laws.

34. Another phænomenon led me ſoon after to a more determined theory in reſpect of thoſe two oppoſite effects of *moiſture* on *threads*. I have ſaid above, that *hemp* and *gut* have only a very little *retrogradation*; their greateſt difference from the *ſlips* conſiſting in their being *ſtationary*, while the *ſlips* have ſtill great motions. But when theſe ſame *threads* are *twiſted*, they acquire a very ſenſible *elongation* beyond their point of *extreme moiſture* ſucceeded by *retrogradation*. From ſeveral trials I have made in *twiſting* theſe *threads* more and more, I do not conſider as impoſſible, if ſome difficulties, which I only could obviate in part, were completely prevented, that they might be brought to ſuch a ſtate, as to have their point of *extreme* dryneſs coincide with that of *extreme moiſture*; by which means, in the progreſs of *moiſture* from one *extreme* to the other, they would move firſt in one direction with decreaſing ſteps, then in the oppoſite direction by increaſing ſteps: the whole however

with great irregularities. Here then we ſee two *oppoſite* effects of *moiſture*; one which *lengthens* the *fibres*; the other which, by *ſwelling* the *twiſted* ſtrings, *ſhortens* them; and we ſee thoſe effects follow different *laws*, from which is produced a *retrogradation* that we may change *ad libitum*.

35. Now, the texture of animal and vegetable *fibrous* ſubſtances muſt be a ſort of *reticle*, which exiſts in thoſe which are naturally in thin *threads*, and in the moſt minute *faſciculi* that we can ſeparate from a maſs; and we ſee it in the laſt caſe, for in ſubdividing thoſe *faſciculi*, there are always *fibres* breaking in the points where they were anaſtomoſed with others; conſequently, the primary *fibres* of thoſe ſubſtances form between them *meſhes* ſimilar to thoſe of a *net*; and thoſe *meſhes*, which are *widened* by the introduction of *water*, muſt produce in the *threads* the ſame effect as the *twiſt* in the above *ſtrings*.

36. If then *moiſture*, in acting on vegetable and animal *threads*, natural and artificial, produces on their *length* two oppoſite *effects*; one of which, ſmall at firſt but increaſing gradually, *compenſates* at ſome period the other which is firſt viſible, and *ſurpaſſes* it afterwards, ſooner or later, according to the nature of the *threads*; it is evident, that they cannot be proper for the *hygrometer*; ſince, from the indication of ſome of them it might ſometimes be concluded, that *moiſture* changes in one ſenſe, while it really changes in the contrary ſenſe; and from ſome others, that *moiſture* is *extreme*, long before it is really ſo. As for the *ſlips*; ſince *moiſture* has only one effect on their *length*, that of *widening* more or leſs the *meſhes* of the *croſs fibres*, I concluded; that all their *hygroſcopic* indications, in every part of their ſcale, were *true* in reſpect of *increaſe* and *decreaſe* of *moiſture*; and that conſequently, that claſs of *hygroſcopes* might be depended upon on that important

point. As for the exact *ratio* between the *indications* of thoſe laſt *hygroſcopes*, and the *changes* of *moiſture*, that was to be the object of a particular inquiry, to which I now come.

Of the ſcale *of the* hygrometer *between the two* fixed points.

37. The long attention I had formerly given to the comparative *expanſions* by *heat* of various kinds of *liquids* and *ſolids*, made me expect the variety I afterwards found in the modifications of *hygroſcopic* ſubſtances by *moiſture*; therefore, as I expreſſed it in § 2. of my firſt Paper, my view was only at that time, to find ſome means of producing a ſteady *comparable* hygrometer; but afterwards I pointed out, in § 72, a means which had occurred to me, for attempting to find the *ratio* between the *expanſions* of ſome determined *hygroſcopic* ſubſtance, and the correſpondent *increaſes* of *moiſture*; which was, to compare the firſt with the correſpondent changes of *weight*, of the ſame or of any other ſubſtance; an idea which I did not then much ſcrutinize, not yet thinking of its execution.

38. From what I have ſaid above, I did not want any other motive of choice between the *ſlips* and the *threads* than their comparative *marches*; but though the *ſlips* agree always in the *direction* of their motions, there are differences in the progreſſion of their comparative ſteps; and that difference led me to examine more attentively the above means of finding which of thoſe *marches* agreed beſt with that of *moiſture*. The reſult of that examination was diſtruſt, at leaſt in reſpect of an immediate deciſive means. With the view of rendering the changes of *weight* more eaſily meaſurable, I had firſt thought of ſome ſubſtance poſſeſſed of a ſtrong *affinity* with *water*; but on con-

ſidering the *hygroſcopic* phænomena of thoſe ſubſtances, it appeared to me, that their changes of *weight* could not be proportional to the degrees of *moiſture* in the *medium*; and that even the ſenſe of the word *moiſture* applied to them was very difficult to determine (§ 14.). As for the ſubſtance of the *hygrometer* itſelf, I did not find any reaſon to think, that its changes of *weight* could be more proportional to *moiſture* than its degrees of *expanſion*; ſince on theſe laſt depended in part the quantity of *water* that could be admitted into its pores at each degree of *moiſture* in the *air*.

39. Being thus diſappointed in my firſt ſcheme, I thought of a more direct method, which was to act on *moiſture* itſelf, by firſt producing, in a glaſs veſſel containing an *hygroſcope*, as much *dryneſs* as I could conceive poſſible at that time, and then introducing into it ſucceſſive equal quantities of *water*, for which I had found a ſure means without opening the veſſel. But then again ſome previous experiments deſtroyed my confidence in that method; having found,—1ſt, That the evaporated *water* had a tendency to depoſit itſelf againſt the *glaſs* by the ſmalleſt difference between the inſide and outſide *temperatures*, even to the degree of becoming viſible on ſome part of the veſſel, long before I had any reaſon to expect *extreme moiſture*.—2d, That by the common *temperature* of my room, the *hygroſcope* in the veſſel remained always at a conſiderable diſtance from its point of *extreme moiſture*, though the bottom of the veſſel was covered with *water*; and that it varied with the *temperature*; which could not have happened if *moiſture* had been *extreme*.—3d, That when I endeavoured to increaſe *moiſture* in the veſſel by cooling it, I produced very often the contrary effect, at the ſame time that a quantity of the diſſeminated *water* gathered over the glaſs.

40. As in thoſe trials the contradiction between the *marches* of the *ſlips* and the *threads* was very evident, I was the more diſappointed to find, that the uncertainty of the real degrees of *moiſture* increaſed at that very period: for inſtance, ſetting out from ſome *dry* point, and *moiſture* increaſing, the *march* of a *thread* of *whalebone* was evidently in a *decreaſing* progreſſion, comparatively to that of a *ſlip* of the ſame ſubſtance; and when at laſt there was a ſuperfluous quantity of *water* in the veſſel, and the *temperature* was made to change, the *thread* was almoſt *ſtationary*, while the *ſlip* had conſiderable motions, often contrary to the ſmall motions of the other; while it was at that very period, that the real degrees of *moiſture* in the veſſel could not have been aſcertained but by an already ſettled *hygrometer*. I had no doubt that thoſe anomalies were to be attributed to the *thread* of *whalebone*, not only becauſe of the exceſs of the ſame modification in other *threads*; but alſo conſidering the analogy between the comparative *marches* of the two kinds of *hygroſcopes*, and thoſe of the *thermometers*, of *water* and *quickſilver*: but as this was a very important object for natural philoſophy, I would not decide it from thoſe firſt appearances; and that conſideration led me to a very great number of various ſorts of experiments, which I made in view of multiplying at leaſt the indirect facts with which my theory might be compared. However, as at laſt I found a more direct means of verification, I ſhall only mention that laſt claſs of experiments.

Experiments on the comparative changes of weight *and* dimenſions *of ſome* hygroſcopic ſubſtances.

41. I have ſaid above, that I could not find any ſolid reaſon to conſider the changes in *weight* of a ſubſtance, as being more proportional, than its changes in *dimenſions*, to the correſpondent changes of *moiſture* in the *medium*; which doubt had prevented me from undertaking that courſe of experiments. But it occurred to me at laſt, that if my theory on the comparative *marches* of the ſlips and the threads were true, it might be rendered certain by comparing thoſe marches with the increaſe of *weight* of the ſame ſubſtance: for inſtance, taking a *ſlip* and a *thread* of *deal*, and having ſome *deal* hung to a ſcale, I was to find, that while the *ſlip* continues to lengthen, and the thread to ſhorten, the ſubſtances continue to receive *water*; and, in general, that the *march* of *ſlips*, in every part of their ſcale on which the experiment may be regular, was more proportional, than that of *threads*, to the correſpondent changes in *weight* of every *hygroſcopic* ſubſtance of the *elaſtic* kind.

42. This having ſtruck me as a ſure means of deciding the queſtion, I ſet immediately to work; and ſince that time I have made a great number of experiments of that kind. They were not at firſt very accurate; but ſucceſſively I have mended both the inſtruments and the apparatus; and, after having ſettled every part that I looked upon as eſſential, and made in conſequence a new apparatus and new inſtruments, I have begun a regular courſe of experiments, of which I ſhall give here the firſt reſults.

43. The apparatus conſiſts in two tin veſſels; the firſt of which, and the moſt uſed, is 16½ inches high, 15½ wide, and 5 deep. The front of this veſſel is a plate of glaſs, and the back a tin-plate *ſlider*, which, being taken off, leaves that ſide

of the veſſel quite open. The ſecond veſſel has the ſame dimenſions as the firſt; but its back is ſoldered, and its front is of woven braſs wire. This veſſel may be applied to the back of the former, in ſuch manner as to make of both one ſingle veſſel, which, when the *ſlider* of the fore-part is taken off, is only divided by a vertical partition of woven braſs wire. The uſe of that ſecond veſſel is to produce *extreme dryneſs* in the other; for which purpoſe it is filled with large pieces of *quick-lime* taken from the kiln. When that veſſel is not uſed, it is kept in a tin box which it fills intirely; and when it is in, as well as while it is out for uſe, that box is kept ſhut with putty, by which means the ſame *lime* may ſerve many times in the following manner.

44. When I want to produce *extreme dryneſs* in the firſt veſſel, I apply to it the ſecond, faſtened with hooks; I then pull out the *ſlider* of the firſt, and ſtop with putty the chinks between them. When that firſt operation is completed, I put again the *ſlider* to the fore-veſſel, and take off the other. In this laſt operation, ſome *moiſture* might be introduced through the chinks of the ſlider, before they are again ſtopped with putty; eſpecially as the deſtruction of *moiſture* in the veſſel has made room for more *air* to come in; but I prevent it by making firſt the apparatus ſenſibly warmer than it was when I put on the lime-veſſel; by which means, in the little time employed for the operation, the motion of the air is from the inſide to the outſide, which prevents all acceſs of *moiſture* in the veſſel.

45. To that firſt operation ſucceeds that of a gradual introduction of *moiſture* into the apparatus. It would have been uſeleſs to apply to that proceſs the means I had imagined, for ſpreading ſucceſſive equal quantities of water in a cloſe veſſel; for the evaporated *water* depoſiting itſelf in part, more or leſs, on every ſurface, and the ſurfaces, eſpecially of *glaſs*, being

very much multiplied in that apparatus when filled with inſtruments, no real correſpondence could be expected between the quantity of evaporated *water* and the motion of the inſtruments; therefore I again gave up the view of determining that *ratio*, and proceeded in the following manner. At the bottom of the apparatus, on one of its ſides, there is an opening, ½ an inch high, and 2½ inches broad, which uſually is kept ſhut with a tin plate and putty. The taking off, only for a moment, that tin plate, is the firſt operation by which I introduce *moiſture* into the apparatus; which being then reduced to the *temperature* at which I make all my obſervations, namely, 60° of FAHRENHEIT, permits the external air to enter the veſſel, and come to an *equilibrium* with it at the ſame temperature. By keeping off the tin plate longer and longer, I admit new quantities of *moiſture* into the veſſel; and when that means is become ineffectual, I introduce through the ſame opening, a braſs frame, which extends under all the inſtruments, on which is ſtretched a cloth that I *wet* by degrees more and more, as long as it may produce ſome effect at the above *temperature*. In order to have that *temperature* when I want it, I make thoſe experiments in a ſeaſon when I may have fire in a ſtove at a proper diſtance from the apparatus. The time when that equal *temperature* is neceſſary in many reſpects, is that of the obſervations, which I make twelve hours after each new introduction of *moiſture*.

46. The inſtruments I place in that apparatus are of two kinds; the firſt of which are *beams*, made on the principle uſed by Mr. JOHN COVENTRY in his *paper hygrometer*, which principle I have found of great uſe; for, with *beams* of that ſort, as delicate as mine are, if the total change of weight in an experiment is not above 1 grain, $\frac{1}{1,000}$ part of it may be diſtinctly obſerved on a weight from 3 or 4 to 20 grains; but in my experiments,

in which the total variation was from 5 to 6 grains, the obſervable part was only $\frac{1}{160}$ part of a grain. Theſe two *beams* are placed on the ſame line through the middle of the depth of the veſſel, and their indices move in that plane; their motions are in an oppoſite ſenſe by the ſame changes of weight, becauſe I wanted the two ſubſtances ſuſpended to the *beams*, to hang near one another in the middle of the veſſel. The other inſtruments are *frames*, in which an *index* is moved by the variations in *length* of a very thin *ſlip* or *thread*. Theſe frames are placed before and behind the *beams*.

47. In the firſt experiment that I made with that apparatus, the ſubſtances ſuſpended to the *beams* were *deal* and *quill*, reduced to very thin *ſhavings*, which were ſtretched edgewiſe in thin braſs-wire frames. The weight of each kind of theſe *ſhavings* was 12 grains, at a certain degree of the thermometer and of my hygrometer. The other *hygroſcopes* were *ſlips* and *threads* of the ſame ſubſtances as the *ſhavings*, and alſo of *whalebone*. Theſe ſix laſt inſtruments have their point of *extreme dryneſs* taken in my *lime-veſſel*, and their point of *extreme moiſture* in *water*; the interval between theſe points is divided into 100 parts; and on the *ſcales* of the *threads* the *degrees* are prolonged beyond this laſt point. The *hygroſcopic* ſcale of the *ſhavings* could not be fixed before the operation; therefore the ſcales of the beams ſerved only to indicate the comparative motions of the index; but afterwards, taking for 0, the point where the *index* ſtood by *extreme dryneſs*, and for 100, the point of *extreme moiſture*, which I ſhall explain, the interval between theſe two points became a *modulum* by which I have alſo reduced into 100 parts of the *whole* the changes obſerved in the *weight* of the *ſhavings*. I ſhall not give here the abſolute quantities, either of the changes of *weight*, or

of thoſe in *length* of the other *hygroſcopes*, having not had time to make the neceſſary calculations, of which however I have the *data*.

48. The neceſſary time for a complete diffuſion of the newly introduced *moiſture* in the veſſel renders it impoſſible to proceed, in that introduction, by regular ſteps. The method I uſe is, to obſerve the motion of my uſual *hygrometer*, which is a *ſlip* of *whalebone*, and to remove the cauſe of increaſe of *moiſture* before it has moved 5 degrees. In that manner the ſteps of the increaſing *moiſture* have been in general leſs than 5 degrees of that inſtrument; but, by interpolation, I have reduced them to what they would have been if the ſame inſtrument had been moved ſucceſſively 5 degrees.

49. I have ſaid before, that when the *maximum* of *evaporation* is produced in a cloſe veſſel by a *temperature* ſenſibly above the *freezing point*, there is no regularity to be expected in any farther attempt to increaſe *moiſture*; the diſſeminated *water* being then abundant, the ſmalleſt difference of temperature between different parts of the apparatus makes it depoſit itſelf on ſome ſurface, and paſs from one to the other (§ 39.); which circumſtance is alſo mentioned by Profeſſor PICTET of Geneva, in his late work, *Eſſais de Phyſique*. For that reaſon, when the *evaporated water* in the veſſel was near its *maximum*, my laſt operation was, to put in again the *wet* cloth, while I kept the *temperature* at 60°, and to take it out when the *indices* of the *beams* were fixed. My uſual *hygrometer* was then at 87°; and as it had ſtill 13 *degrees* to move towards its point of *extreme moiſture*, and all the others in proportion of their known *marches*, I have added to the obſerved increaſes of *weight* in the *ſhavings*, a quantity proportional to their former marches

comparatively to that of their reſpective *ſlips*; and thus are completed their *hygroſcopic ſcales*. Repeated trials have ſhewn me, that the *weight* of the *ſhavings* increaſes as long as their *ſlips* increaſe in *length*; but as there is no regularity in their comparative *marches* at that period of *moiſture* in a veſſel, nor any poſſibility of making theſe experiments in open air, becauſe of the *beams*; the addition mentioned, which forms the three laſt *terms* of the columns of the *ſhavings* in the following table, is to be conſidered only as having determined the *modulum* of the *obſerved* terms; ſince it has not changed the *ratio* between them, nor conſequently the correſpondent *marches* of *weight* and *length* ſo far; which were the only object of the experiment.

50. Before I come to the general reſult of that experiment, I ſhall place here a comparative view of the two kinds of phænomena, which, by their analogy, led me firſt to the theory I have expoſed: I mean the comparative *marches* of the *ſlip* and *thread* of *whalebone* on one ſide, and thoſe of the *thermoſcopes* of *quickſilver* and *water* on the other ſide. In this table the correſpondent *terms* of the *hygroſcopes*, from 0 of both to 85 of the *ſlip*, have been obſerved in the above experiment; the four following are the reſults of obſervations in time of increaſing *dew*. The correſpondent *terms* of the *thermoſcopes* are deduced from the *table* of their comparative *expanſions* which I have given in § 418. *m.* of my work, *Rech. ſur les Mod. de l'Atmoſphère*; from which *table* this only differs, 1ſt, by a change of the *modulum*, in the *ratio* of 80 to 100; 2d, by an *inverſion*, which brings the *terms* of this table to expreſs comparative *condenſations* of the two *liquids*.

HYGRO-

	HYGROSCOPES.				THERMOSCOPES.		
	Whalebone Slip.	Whalebone Thread.			Quickſilver.	Water.	
Extreme	0	0,0	*dryneſs.*	*Boiling*	0	0,0	*point.*
	5	12,1			5	9,3	
	10	30,1			10	18,3	
	15	41,1			15	26,3	
	20	51,1			20	35,0	
	25	59,1			25	42,7	
	30	65,6			30	49,2	
	35	71,1			35	56,7	
	40	76,5			40	63,1	
	45	81,8			45	69,0	
	50	85,8			50	74,5	
	55	88,8			55	79,1	
	60	91,3			60	83,8	
	65	93,3			65	87,9	
	70	95,6			70	91,8	
	75	97,6			75	95,0	
	80	98,6			80	97,5	
	85	99,6			85	98,9	
	90	100,1			90	99,9	
	95	100,5			95	100,5	
Extreme	100	100.	*moiſture.*	*Freezing*	100	100.	*point.*

51. The firſt part of that table ſhews, the great ſteps of the *thread* of *whalebone*, comparatively with thoſe of the *ſlip* of the ſame ſubſtance, at the beginning of their correſpondent increaſe in *length*; but the *thread* relents by degrees, and a nearly *ſtationary* ſtate ſucceeds, in which, while this *hygroſcope* moves firſt 1,9 *degree* from 98,6 to 100,5, then *retrogrades* 0,5

degree to come to its point of *extreme moiſture*; the *ſlip*, continuing to move in the former direction, goes over 20 *degrees*. The phænomena are the ſame in the part of the table relating to the *thermoſcopes*; that of *water* proceeds alſo at firſt by great ſteps, comparatively to that of *quickſilver*; after which it relents, and while it moves only 3 degrees, and *retrogrades* 0,5 degree to come to the *freezing point*, the *quickſilver* one, continuing to move in the ſame direction, goes over 20 degrees. It was from that phænomenon of the *water thermoſcope* that I concluded formerly, that there was in its *march* a *ſtationary* ſtate, during which the *heat* decreaſed nearly as indicated by the *quickſilver* one; and that concluſion was afterwards confirmed by direct experiments. From that aſcertained fact, I was led to conclude, with reſpect to moiſture, that there was alſo a *ſtationary* ſtate in the *march* of hygroſcopic *threads*, even in thoſe which had the ſmalleſt *retrogradation*, as that of *whalebone*; and this theory will be confirmed by the reſults of the above-deſcribed experiment.

52. The following table contains thoſe reſults, namely, the correſpondent *marches* of all the mentioned *hygroſcopes*; the *ſhavings* increaſing in *weight*, and the *ſlips* and *threads* in *length*. The 3 laſt comparative *terms* do not reſult from that particular experiment; for the *ſhavings* (as I have ſaid above) they are concluded from the former comparative ſteps; for the other inſtruments, they have been obtained by obſervations in the open *damp* air.

	WHALEBONE.		QUILL.			DEAL.		
	Slip.	Thread.	Shavings.	Slip.	Thread.	Shavings.	Slip.	Thr.
Extr. dryn.	0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
	5	12,1	7,0	4,8	40,0	6,2	5,4	42,0
	10	30,1	13,0	9,7	72,0	9,4	11,2	69,4
	15	41,1	20,0	14,4	85,0	15,6	16,5	94,8
	20	51,1	26,0	19,2	95,0	22,6	21,9	107,0
	25	59,1	31,0	23,9	101,0	27,0	27,2	113,6
	30	65,6	36,0	28,5	105,0	33,2	32,7	118,6
	35	71,1	42,0	33,3	107,0	36,0	38,3	122,6
	40	76,5	43,8	38,3	102,0	41,2	43,7	120,6
	45	81,8	48,3	42,9	104,0	46,7	49,2	123,6
	50	85,8	52,3	47,4	107,0	49,7	54,6	126,6
	55	88,8	56,5	52,4	103,0	56,1	59,9	119,7
	60	91,3	60,5	56,9	105,0	59,9	64,9	122,7
	65	93,3	64,4	61,9	106,0	63,7	69,7	119,7
	70	95,6	69,4	67,2	108,0	67,1	74,5	117,6
	75	97,6	74,0	72,2	107,0	73,4	79,0	115,6
	80	98,6	78,0	77,8	106,0	78,1	83,5	112,6
	85	99,6	84,0	82,8	105,0	83,8	87,5	110,0
	90	100,1	* 88,0	88,2	103,6	* 88,8	92,0	107,0
	95	100,5	* 94,0	94,0	102,0	* 93,8	96,0	103,6
Extr. moiſt.	100	100.	* 100.	100.	100.	* 100.	100.	100.

From the 18 firſt *terms* of that table, which are the immediate reſults of the experiment, we are now to examine my opinion, that the *lengthening* of the *ſlips* of *whalebone*, *quill*, and *deal*, beyond theſe *terms* is a ſure ſign that, till they have attained their point 100, *moiſture* continues to increaſe in the *medium* where they are placed.

53. There could not be any doubt on that propoſition, if it were not for ſome *threads* ſimilar to that of *whalebone*; which *threads*, having ſenſibly attained their utmoſt *length* at the period when the experiment was ſtopped, ſeem to indicate, that

moiſture is then at its *maximum.* But if the *lengthening* of *hygroſcopic threads* in general, is the compound effect of two oppoſite cauſes which follow different laws; it may be that, in ſome *threads*, thoſe cauſes happen to compenſate each other at that very period; by which means they are *ſtationary*, though *moiſture* continues to increaſe. This was my opinion, and the above experiments were undertaken to verify it, firſt, by comparing the *march* of the *ſlips* with the increaſe in *weight* of their own ſubſtance; ſecondly, by comparing the *marches* of different kinds of *threads* with each other, and alſo with the increaſe in weight of their ſubſtance: and from that now we are to examine the above propoſition.

54. In reſpect of the *ſlips* my theory is, that as *moiſture* cannot act on their length but by widening the *meſhes* of their tranſverſal *fibres*, they cannot go on *lengthening* but by imbibing more and more *moiſture*, from its increaſe in the air; and this we ſee to be the caſe, by comparing the *marches* of the three kinds of *ſlips* with the correſpondent increaſes in *weight* of the *quill* and *deal* ſhavings, during the whole progreſs of the experiment. There are differences in thoſe *marches* as I expected (§ 37.); but they are not ſuch as to give the ſmalleſt reaſon to ſuſpect, that afterwards, during the period of the three laſt *terms* of the table, in which we have no correſpondent obſervations of increaſes of *weight* in the *ſhavings* of *deal* and *quill*, the ſame law does not take place as in the 17 antecedent *terms.* If the experiment was only made with one kind of *ſlip*, it might be objected, that though that *ſlip* lengthens regularly during the whole increaſe of *moiſture* from its *minimum* to its ſuppoſed *maximum*, it is not impoſſible but that immediately after, by ſome peculiarity of its nature, it will *lengthen*, without any farther increaſe of *moiſture* in the

medium. But that ſurmiſe cannot be admitted when the *ſlips* of ſuch diſſimilar ſubſtances as *whalebone*, *quill*, and *deal*, ſenſibly agree in their *motions* at that period, and when a number of other *ſlips* of the vegetable and animal kinds follow alſo the ſame general *march.*

55. In reſpect of the *threads*, which are the only cauſe of the above doubt, my theory, which removes it, is alſo confirmed by that experiment. That cauſe of doubt is exemplified in the table by the *thread* of *whalebone*, which has almoſt no motion, while its *ſlip* moves from 85 to 100. At that period of *moiſture*, no regularity can be expected from experiments made in *cloſe veſſels*; by which circumſtance, not having the correſpondent obſervations of *weights*, it cannot be demonſtrated by immediate experiments, that the *thread* of *whalebone* is then *ſtationary* from its nature; but the *threads* of *quill* and *deal*, which are in that ſtate during the regular courſe of the experiment, will guide us in that enquiry. The *thread* of *quill* is *ſtationary* during that great part of the obſerved increaſe of *moiſture*, by which the *thread* of *whalebone* moves from 71 to 97,6: the *thread* of *deal* is alſo *ſtationary*, while the ſame *thread* moves from 71 to 91,3. They both afterwards *retrograde*; the *quill* from 107 to 100, and the *deal* from 122,6 alſo to 100; and it is during the latter part of that *retrogradation*, correſpondent to a continued *direct* motion of the *ſlips*, that the *thread* of *whalebone*, and ſome more of its claſs, after a very *decreaſing* march comparatively with all the *ſlips*, are at laſt *ſtationary*, and then a little *retrograde*. In that *ſtationary* ſtate of the *threads*, while moiſture proceeds in the ſame direction, they move backwards and forwards, more or leſs, according to the duration of that ſtate, and to the quantity of the *retrogradation*. This may be ſeen by the table, in

reſpect of the *thread* of *deal* and *quill*; and I have obſerved it conſtantly, in a ſmaller proportion, in all the *threads* which have their *ſtationary* ſtate at the laſt period of *moiſture*, with this particular circumſtance in all thoſe motions backwards and forwards, that they are never the ſame in two different experiments. That ſymptom already points out a complication of cauſes; but we ſhall ſoon ſee a more diſtinct proof of their exiſtence.

56. My theory on the *march* of hygroſcopic *threads* is founded on this general principle, that a *retrograde* effect, how ſmall ſoever it may be, if it is not produced by a correſpondent change in the cauſe itſelf, is preceded by a *ſtationary* ſtate, during which, and the *retrogradation*, the intenſity of the cauſe continues to *increaſe*: and this alſo is exemplified in the experiment. The *ſtationary* ſtate of the *thread* of *deal* begins, when its *ſhavings* have only imbibed from the air a quantity of *water* = 36; it is ſtill at the ſame point when the quantity of imbibed *water* has increaſed to 59,9; and in the part of its *retrogradation*, which is ſtill contained in the regular courſe of the experiment, that quantity of imbibed *water* increaſes to 88,8. The ſame, with only ſome differences in the degree, is ſeen in the *thread* of *quill*; therefore, as we ſee alſo ſome, though a very ſmall, *retrogradation* in the *thread* of *whalebone*, as well as in other *threads* of the ſame *claſs*, we have reaſon to conclude, that their apparent immobility before they come to that point, while the *ſlips* continue to move, is alſo a *ſtationary* ſtate, during which they continue to receive water, by the increaſe of *moiſture* in the air.

57. The experiments I have now analyſed are only one ſet amongſt others which, though made with leſs accuracy, have given the ſame general reſults. Theſe, relating to various

kinds of ſubſtances, I intend to repeat, and to have the honour of communicating their reſults to the Royal Society; and I ſhall conclude this Paper, with an immediate demonſtration, that the *hygroſcopic* motions of the *ſlips* are ſimple, while thoſe of the *threads* are the combined effects of two oppoſite cauſes: which will be a further confirmation of the whole of the above theory.

On the recoil *of hygroſcopic* threads.

58. When formerly I concluded from the phænomena of the *water thermoſcope*, that its *condenſations* were the combined effects of two oppoſite cauſes, which followed different laws, it was not for having diſtinguiſhed thoſe two effects; but only becauſe of a ſmall *retrogradation* near the *freezing point*, preceded by a *ſtationary* ſtate, comparatively with the *march* of *quickſilver*; but in the caſe of hygroſcopic *threads* and *ſlips*, in which we have the ſame phænomenon, the two oppoſite effects are diſtinguiſhable in the *threads*, by one being operated more rapidly than the other. If, for inſtance, I tranſport from a drier to a damper place (or inverſely) the two kinds of *quill* hygroſcopes, the *ſlip* proceeds in an *even* courſe to a certain point, where it remains *fixed*; but the *thread* moves in an *interrupted* manner alſo to a certain point, whence it *recoils*. If that experiment is made within the limits of the *ſtationary* ſtate of the *thread*, it may *recoil* as much as it has gone the other way, and be fixed at the ſame point in both places. The caſe of the *ſlip* of *quill* is common to every *ſlip*, and that of its *thread* to all others which have a quick motion. Here then we have *ſeparately* the *two* effects of *moiſture* on the *threads*; that on the *fibres* themſelves is the *ſooneſt* produced, and at firſt predominates: the *ſloweſt*,

by which afterwards the firſt produced is more or leſs *compenſated*, is that operated on the width of the *meſhes*; and it is becauſe the laſt of thoſe effects is the only one that can affect the *length* of the *ſlips*, that, in every change of *moiſture*, they move *evenly*, without any *recoil*.

59. To that demonſtration of the exiſtence of *two* oppoſite *effects* of *moiſture* in the *threads*, I ſhall now only add an example of a ſimilar phænomenon, in which the *cauſes* alſo are viſible. The *compound frames*, mentioned in § 24. are formed of two *glaſs* rods 4 feet long, fixed together at the bottom and the top. A thin ſlip of *braſs* of determined length, fixed to one of the *glaſs* rods towards the top, comes down from thence, paſſes over a pulley at the bottom, and turns up for half an inch. To this end of the *braſs* ſlip is fixed the lower end of the hygroſcopic ſubſtance, the upper end of which is connected with the *index* of the inſtrument. It is by that means that, whatever be the changes of *heat*, provided they are *ſlow*, the lower end of the hygroſcopic ſubſtance remains ſenſibly at the ſame diſtance from the axis. But if I take that inſtrument out of water at a low *temperature*, and plunge it immediately into *warmer* water, the *index* inſtantly moves as if the hygroſcopic ſubſtance had lengthened; which is the *effect* of the *braſs* ſlip dilating ſooner than the *glaſs* rods; then the index *recoils*, and this is the *effect* of a *ſlower* dilatation of the *glaſs*.

Concluſion.

60. I have concentrated in theſe pages an account of twenty years aſſiduous labour in *hygrometry*, moſtly occaſioned by the anomalies of the *hygroſcopic threads*; and the principal reſults have been, ſome determinations of the four principles that

directed me from the beginning, which now are as follows. 1ſt, *Fire*, as cauſe of *heat*, is a ſure, and the only ſure, means of obtaining *extreme dryneſs:* this is produced by *white heat* in every *hygroſcopic* ſubſtance that can bear it; and it may be thus tranſmitted to the *hygrometer*. 2d, *Water*, in its liquid ſtate, is a ſure, and the only ſure, means of determining the point of *extreme moiſture* on that inſtrument. 3d, It is not to be expected, *à priori*, of any *hygroſcopic* ſubſtance, that its *changes* be proportional to thoſe of *moiſture*; but it may be affirmed, that no *fibrous* or *vaſcular* ſubſtance, taken *lengthwiſe*, is proper for the *hygrometer*. 4th, A means of throwing light on the *march* of a choſen *hygrometer*, may be, to compare it with the correſpondent changes in *weight* of many *hygroſcopic* ſubſtances.

61. From thoſe determinations in *hygrometry* ſome great points are already attained in *hygrology*, *meteorology*, and *chemiſtry*, of which I ſhall only indicate the moſt important. 1ſt, In the phænomenon of *dew*, the *graſs* often begins to be *wet* when the *air*, a little above it, is ſtill in a middle ſtate of *moiſture*; and *extreme moiſture* is only certain in that *air*, when every ſolid expoſed to it is *wet* (§ 28.). 2d, The *maximum* of *evaporation* in a cloſe ſpace, is far from identical with the *maximum* of *moiſture*; this depending conſiderably, though with the conſtant exiſtence of the other, on the *temperature* common to the *ſpace* and to the *water* that evaporates (§ 30.). 3d, The caſe of *extreme moiſture* exiſting in the open tranſparent air, in the day, even in time of *rain*, is extremely rare: I have obſerved it only once, the *temperature* being 39°. 4th, The *air* is *dryer* and *dryer* as we aſcend in the atmoſphere; ſo that in the upper attainable regions, it is conſtantly very *dry*, except in the *clouds*. This is a fact certified by M. DE SAUSSURE's obſervations and mine. 5th, If the whole atmoſphere paſſed

from *extreme dryneſs* to *extreme moiſture*, the quantity of *water* thus *evaporated* would not raiſe the *barometer* as much as half an inch. 6th, Laſtly, in chemical operations on *airs*, the greateſt quantity of *evaporated water* that may be ſuppoſed in them at the common *temperature* of the atmoſphere, even if they were at *extreme moiſture*, is not ſo much as $\frac{1}{100}$ part of their maſs. Theſe two laſt very important propoſitions, have been demonſtrated by M. DE SAUSSURE.

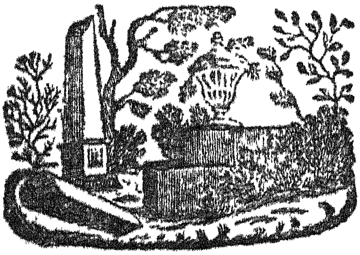


Fig 1

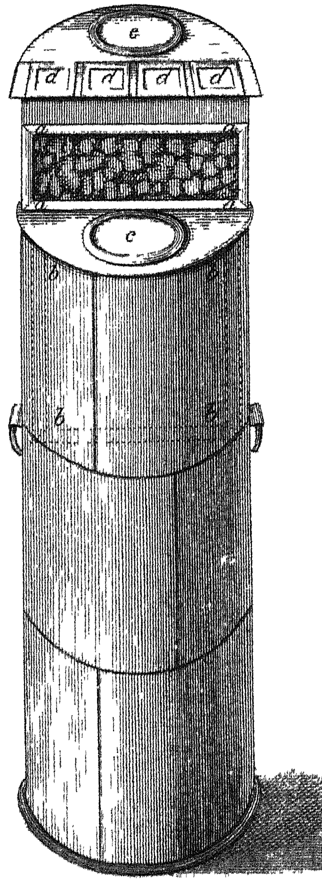
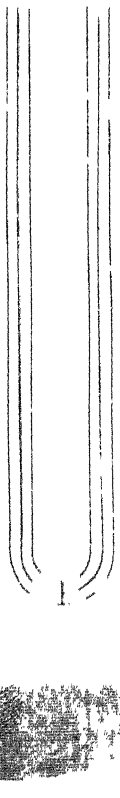


Fig 2



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II. *On the Production of Ambergris. A Communication from the Committee of Council appointed for the Conſideration of all Matters relating to Trade and Foreign Plantations; with a prefatory Letter from* William Fawkener, *Eſq. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 20, 1791.

TO SIR JOSEPH BANKS, BART.

Office of Committee of Privy Council for
Trade, Whitehall, 15th January, 1791.

SIR,

LORD HAWKESBURY, Preſident of the Committee of Privy Council appointed for the Conſideration of all Matters relating to Trade and Foreign Plantations, having received a Letter from Mr. CHAMPION, a principal Merchant concerned in the Southern Whale Fiſhery, informing him, that a ſhip belonging to him had lately arrived from the ſaid fiſhery, which had brought home 362 ounces of ambergris, found by Mr. COFFIN, captain of the ſaid ſhip, in the body of a female ſpermaceti whale, taken on the Coaſt of Guinea; his Lordſhip thought fit to deſire Captain COFFIN, as well as Mr. CHAMPION, to attend the Lords of the Committee, that they might be examined concerning all the circumſtances of the fact before mentioned; and I am directed by their Lordſhips to tranſmit to you a copy of the examination of theſe two gentlemen, that you may communicate the ſame to the Royal Society, if you ſhould think that any of the circumſtances, ſtated in this examination, will contribute to remove the doubts hitherto entertained concerning the natural hiſtory and production of this valuable drug. I ſend you alſo a piece of the ambergris ſo taken out of the whale, and ſome of the bills of the fiſh called ſquids (which are ſuppoſed to be the food of ſpermaceti whales), and which were found partly in the ambergris taken from this female whale, and partly on the outſide of it, and adhering to it.

I have the honour to be, &c.

W. FAWKENER.

At the Council Chamber, Whitehall, the 12th January, 1791.

By the right honourable the Lords of the Committee of Council appointed for the Conſideration of all Matters relating to Trade and Foreign Plantations.

READ—Letter from Mr. ALEXANDER CHAMPION, a principal merchant concerned in the Southern Whale Fiſhery, to Lord HAWKESBURY, dated the 2d inſtant, acquainting his Lordſhip, that Captain JOSHUA COFFIN, of the ſhip The Lord Hawkeſbury, is lately arrived from the Southern Whale Fiſhery; and that the ſaid ſhip, beſides a cargo of 76 tons of ſpemaceti oil and head-matter, has brought home about 360 ounces of ambergris, which the ſaid captain took out of the body of a female ſpermaceti whale on the Coaſt of Guinea.

Meſſ. CHAMPION and COFFIN attending, were then called in, and the following queſtions were put to Mr. COFFIN, *viz.*

Q. Have any of the whales, taken before by ſhips ſailing from Great-Britain, to your knowledge, contained any ambergris?

A. None, that ever I heard of. The American ſhips have, at times, found ſome.

Q. Was the ambergris, found by you, in a bull or cow fiſh?

A. It was found in a cow fiſh.

Q. Is it uſual to look for ambergris in whales that are killed?

A. It has not hitherto been much the practice to do ſo.

Q. How happened it that you diſcovered this?

A. We ſaw it come out of the fundament of the whale; as we were cutting the blubber, a piece of it ſwam upon the ſurface of the ſea.

Q. In what part of the whale did you find the remainder?

A. Some more was in the ſame paſſage, and the reſt was contained in a bag a little below the paſſage, and communicating with it.

Q. Did the whale appear to be in health?

A. No; ſhe did not. She ſeemed ſickly, had no fleſh upon her bones, and was very old, as appears by the teeth, two of which I have. Though ſhe was about thirty-five feet long, ſhe did not produce above one ton and a half of oil. A fiſh of the ſame ſize, in good health, would have produced two tons and a half.

Q. Have you obſerved the food that whales generally feed upon?

A. The ſpermaceti whale feeds, as I believe, almoſt wholly upon a fiſh called Squids. I have often ſeen a whale, when dying, bring up a quantity of ſquid, ſometimes whole, and ſometimes pieces of it. The bills of the ſquid (ſome of which Mr. COFFIN produced) were found, ſome in the inſide, and ſome on the outſide of the ambergris, ſticking to it.

Q. Did you ever find any ambergris floating on the ſea?

A. I never did, but others frequently have.

Q. How long have you been engaged in the whale fiſhery?

A. It is about ſixteen years ſince I firſt entered into it.

Q. What is the general proportion of bull and cow whales you have met with?

A. I believe the proportion to be nearly equal. In my laſt voyage, however, I found only four bulls out of thirty-five whales. I fiſhed upon the Coaſt of Africa between five North and ſeven South degrees of Latitude. I am inclined to think, that the cow whale goes to calve in the low latitudes, which accounts for more cows being found in thoſe latitudes.

Q. Is there any particular ſeaſon when the cow whales calve?

A. I do not know that there is.

Q. Does the bull or cow whale, in proportion to their ſize, produce moſt oil?

A. The cow whale, when big with calf, produces more oil than a bull whale of the ſame ſize; when ſuckling, ſhe produces leſs.

Q. Are the whales uſually found ſingly, or in pairs? or in larger numbers?

A. Uſually in large numbers, which we call Scools, and particularly in the low latitudes. I have ſeen from fifteen to perhaps a thouſand together.

Q. Have you any further information on this ſubject to give the Committee?

A. We have generally obſerved, that the ſpermaceti whale, when ſtruck, voids her excrement; if ſhe does not, we conjecture that ſhe has ambergris in her. I think ambergris moſt likely to be found in a ſickly fiſh; for I conſider it to be the cauſe or the effect of ſome diſorder.

Queſtions put to Mr. CHAMPION.

Q. At what price does ambergris uſually ſell; and at what price did that, taken by your ſhip, ſell?

A. A ſmall quantity had lately ſold at 25*s. per* ounce; but it was then very ſcarce. Mine ſold for 19*s.* 9*d. per* ounce. The whole quantity, found in this whale, was 362 ounces Troy. The people who bought it told me, this was a larger quantity than was ever before brought at once to market. It has been generally ſold at about 4 or 5 pounds at a time.

Q. For the uſe of what country was this ambergris bought?

A. I do not exactly know. It was bought by a broker, who told me, that his principal, who purchaſed about one half, bought it for exportation to Turkey, Germany, and France. The other half was purchaſed by the druggiſts in town.

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III. *Obſervations on the Affinity between Baſaltes and Granite.* *By* Thomas Beddoes, *M. D.; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 27, 1791.

ALL our opinions on the formation of rocks and mountains, except volcanic mountains, muſt of neceſſity reſt upon analogical reaſoning, ſince we have no direct teſtimony concerning their origin. Hence, whatever portion of the mineral kingdom is but little connected with our experience of the action of fire or water, muſt be ſlightly paſſed over, or ſet aſide for future inveſtigation, while the partizans of the two oppoſite hypotheſes, which at preſent divide the philoſophical obſervers of foſſils, fix their whole attention, and lay all the ſtreſs of their arguments, on ſuch particulars as they are able to connect by ſome analogy with the chemical operations in which either fire or water are principally concerned. For this reaſon, baſaltes has been much more the ſubject of diſputation than granite; the former ſpecies of rock offering appearances that coincide in ſome degree with both kinds of chemical proceſſes, while the latter ſeems to ſtand aloof from the experiments that have given birth to our ſciences. We do, indeed, find opinions on the production of granite by one or other of the cauſes above mentioned; but they are generally [*] looſe conjectures, thrown out at random, rather than philoſophical propoſitions, laid down in preciſe

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[*] The only exceptions known to me are, Phil. Tranſ. Vol. LXV. p. 5—47. and Edinburgh Tranſactions, Vol. I. p. 255—257.

terms,

terms, and ſupported by proper evidence. In conſequence of information obtained from various ſources, I have been led to conſider this queſtion in a light ſomewhat new; and although I cannot but expect my concluſions to be controverted, however ſtrong may be my own conviction of their juſtneſs, I am encouraged by the hope of communicating ſome original obſervations on the phyſical geography of our own country, and of bringing forward to public notice ſome facts not generally known among us, though they are ſuch as cannot be overlooked in a theory of granite.

Notwithſtanding the recent objections of Mr. Werner [*], I ſhall aſſume the origin of baſaltes from ſubterraneous fuſion as thoroughly eſtabliſhed by various authors [†]. Several obſervations of my own, which I intend ſoon to offer to the publick, will, I flatter myſelf, corroborate the evidence, though already ſufficiently ſtrong to remove all reaſonable doubt, and add a conſiderable tract to thoſe where the effects of ancient fire have been traced in our times. It may be proper to premiſe, that under the term baſaltes I comprehend that vaſt natural family of rocks which is frequently cracked into regular colonnades, and may be followed in an unbroken ſeries from this perfect form through endleſs modifications to the moſt ſhapeleſs maſs of trapp or whinſtone. Though frequently of an iron-grey colour and uniform texture, this ſpecies of ſtone varies greatly in both theſe characters, even in the ſame rock. In particular, it paſſes, by the moſt inſenſible gradations, both to the porphyries, with which it coincides in appearance, in compoſition, and doubtleſs alſo in origin, and to the *hornſtein* of the Germans; a term including petroſilex and ſeveral ſorts of cloſe grained whinſtone, of which I have found in England varieties with a conchoidal fracture, ſemi-tranſparent at the edges, and in other reſpects faſt approaching * to a ſiliceous nature. Lilleſhall Hill near Shifnal, in Shropſhire, to mention a ſingle inſtance, affords ſuch ſiliceous, beſides *ſemi-granitic*, porphyritic, and common whinſtone, containing agate.

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[*] Bergmänniſches Journal, March 1789. Among the facts adduced by this celebrated mineralogiſt to prove the watery origin of baſaltes, I cannot diſcover any very new or ſtriking. The appearance of baſaltes between ſtrata is his great argument. But the ſame, or a ſimilar appearance had been noticed by Dr. Hutton (Edinb. Tr. I. p. 279.); Whitehurst (Chap. XVI.): Ferber (Italy, p. 51.) mentions the inſertion of lava between calcareous ſtrata; and in Leske (Reiſe durch Sachſen, 4to, 1785, p. 517.), we have baſaltic columns in ſandſtone. Mr. Werner alſo inſiſts upon the gradual approximation of the adjacent ſtratum towards the nature of baſaltes.

[†] Desmarest, in his well-known Paper in the French Memoirs; Raspe, Extinct Volcanos near Caſſel; Faujas, Volcans éteints, &c. and Minéralogie des Volcans; Sir W. Hamilton, Campi Phlegræi, and Phil. Tranſ.; Ferber, Travels through Italy; Dolomieu, in his Account of the Lipari and Pontian Iſles; Leske, *l. c.*; Beroldingen, in his Account of the Quickſilver Mines in the Palatinate, &c.; Hamilton, Letters on the Coaſt of Antrim; Hutton, *l. c.*; Veltheim, Etwas über die Baſalt, Leipſig, 1787; are among the chief of theſe authors.

But baſaltes, of which a right knowledge is conducting us faſt to a juſt theory of the earth, is not leſs connected with granite; *inſomuch that we may trace theſe rocks gradually approaching and changing into one another.* I have myſelf had an opportunity of examining many connecting links in this gradual ſucceſſion; and this opinion, which has ſince been confirmed by other conſiderations, was firſt forced upon me by ſpecimens in great variety from volcanic and baſaltic countries. But as it is a point by far too important to be admitted on the mere authority of any mineralogiſt, I ſhall endeavour to ſupport it by the teſtimony of obſervers, who cannot be ſuſpected of any bias towards ſuch an hypotheſis. The firſt ſtep in the progreſſion appears at the Giant's Cauſeway in Ireland. Many of the pillars there conſiſt of fine-grained, dark-coloured whinſtone; that variety which may be conſidered as moſt perfect, and as equidiſtant from porphyry, petroſilex, and granite; but at the promontory of Fairhead, the character of the ſtone is ſeen to alter, and it has lately been deſcribed as an imperfect kind of granite *. Hence we are led by regular approaches to perfect priſms of granite, accompanied by priſms of common whinſtone, and not leſs obviouſly than the different ranges on the Coaſt of Antrim betraying a common origin. The pillars of Les Rameaux, though they rather incline towards the dark colour and uniform hard ſubſtance; "yet, when broken, are unequal both in colour and texture, "and ſometimes interſperſed with irregular pieces and patches, "as it were, of an heterogeneous hard ſubſtance, which, by "its micæ and ſmall rhomboidal cryſtallizations, much re- "ſembles a ſort of granite I have frequently ſeen. The "maſs on which theſe columns ſtand is of the ſame mixed "character †." Other examples will occur afterwards; and for baſaltiform colonnades of granite it is only neceſſary to refer to Mr. STRANGE's deſcription of Monte Roſſo ‡. The general ſhape of the Euganean hills, as if ſuddenly raiſed by

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* DOLOMIEU, Iſles Ponces, p. 111. 185. &c. deſcribes juſt ſuch lavas.

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* HAMILTON, 2d edition of his Letters, p. 37. ſays, it "reſembles an imperfect compact granite." I have ſpecimens from an hill near Mallwhyd, in the vicinity of Cader Idris, where the texture inſenſibly changes from an uniform whinſtone ground to grains of mica, feldſpath, ſhoerl, and, I think, ſometimes quartz. They are, I fancy, like thoſe denſe lavas of Etna, "Qui, vues à la loupe, laiſſent appercevoir des ébauches de cryſtallization de ſhörl, de quarz, ou de feld-ſpath. DOLOMIEU, *l. c.* p. 182.

† STRANGE, Phil. Tranſ. LXV. p. 13.

‡ Ibid.

the expanſive and effervescent force of heat * from the ſurrounding plain, the lava intermixed with granite, as if both had concreted together, the columns of an uniform texture in the adjacent parts of theſe hills, and the reſt of the phænomena, even then led the author to ſuſpect, "a ſtrong analogy between granites and many particular volcanic concretions."

From the mountain of Eſterelles, in the South of France, on the road from Frejus to Antibes, I have before me granite, gneiſs, and ſpecimens, in which feldſpath and grains of tranſparent quartz are diffuſed through a paſte of the ſame browniſh red colour and texture as the baſaltic columns at Dunbar in Scotland.

Nothing is indeed more common, or more variouſly modified, than foſſils of this intermediate character †. We frequently find a ground of jaſper, and no doubt alſo of different varieties of whinſtone, as will hereafter appear, with feldſpath and ſhoerl at the ſame time imbedded in them ‡; and again with grains of feldſpath and quartz in ſuch a manner as to leave it extremely doubtful, whether the rock ought to be named granite or porphyry §. The varieties of ſuch rocks will conduct us, by eaſy ſteps, from uniform baſaltes through the porphyries to granite. A chemical examination of the baſis of a number of theſe porphyries would be very intereſting; yet I would not reſt the theory of their formation altogether on the reſult of analyſis.

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* The ſight of them impreſſed FERBER alſo with the ſame idea.

† Il n'y a point de naturaliſtes qui ne connoiſſent le genre de roches, placé entre le granit, les gneiſs, et les porphyres, qui tiennent un peu des trois eſpeces. Iſles Ponces, p. 90,

‡ HAIDINGER, Tract quoted below, p. 47.

§ CHARPENTIER (Mineralogical Geography of Saxony, 4to, 1778. in German), p. 50. and elſewhere, finds himſelf in this ſtate of uncertainty.

The ſame ſtratum is perpetually varying in its mixture; and we ſhould not too rigourouſly adhere to the proportion of ingredients diſcovered by the chemiſt in the hundred grains upon which his experiments may chance to be made. The ſenſible qualities, the ſtile of fiſſure, the accompanying foſſils, and the form of whole rocks, when ſurveyed by an experienced eye, are as good criterions of baſaltes as a certain proportion of iron, and the black glaſs which it yields on fuſion. Should the matter of any given rock contain too little iron to be fuſible by the blow-pipe, and yet have other ſtriking features of whinſtone, would this be a ſufficient reaſon to conclude, that its formation has been different? Chemiſtry, if thus ſtrictly followed, would perplex mineralogy, inſtead of reducing it to order. Characters of minerals, purely chemical, would ſeparate thoſe whoſe natural hiſtory is alike, and bring together ſuch as differ widely in their formation.

The late Mr. FERBER's letters from Italy furniſh ſo many facts, conſpiring in one way or another to ſhew the affinity between baſaltes as well as other products of ſubterraneous fire and granite, that whoever reads them with this view will, I am perſuaded, find himſelf more intereſted and inſtructed. The following are among the moſt ſtriking of theſe facts.

"4th ſpecies of baſaltes. Oriental baſaltes through which "the conſtituent parts of granite are equably diffuſed. Separate particles of red feldſpath, quartz, and mica, are "diſperſed through the ſubſtance of this ſpecies: they ſeem to "have been diſtributed through an aqueous ſolution, and to "prove, that this ſpecies had rather an aqueous than a fiery "origin." I ſee neither proof nor preſumption in favour of this ſuppoſition; but in a ſeries of ſpecimens, collected with a view to ſhew the tranſition from black baſaltes to granite, this ſpecies and the granite from Eſterelles would form two contiguous links.

"5th Oriental baſaltes, with ſtripes of granite. The "common black baſaltes, faſciated with large ſtripes of red "granite, blended and joined to the baſaltes without any "viſible ſeparation; not as the pebbles in a breccia, or as "fiſſures healed up and filled with granite, but as if both the "baſaltes and granite had been fluid together *."

Thoſe ſpecimens, which ſhew how copiouſly volcanos produce feldſpath, ſhoerl, and mica, eſpecially the two former (ſubſtances common both to baſaltes and granite), tend greatly to eſtabliſh the near relation between theſe two kinds of rock. I was ſurprized, at this day, to find an excellent obſerver ſeriouſly maintaining, that theſe earthy cryſtallizations are merely ejected, and not generated, by theſe fires †.

Attempts, I am aware, have been made to ſet up boundaries between the columnar granite of the Euganean hills, the granite of the volcanic provinces of France, the granitello of the Italians, and ſuch granite as is found to conſtitute high and extenſive ranges of mountains. As to a difference in the ſize of particles, and hardneſs of the ſtone, the firſt diſtinction is neither conſtant, nor by any means calculated to perſuade us, that a cauſe, capable of producing the one, is inadequate to the production of the other. It may probably be explained from the quantity of matter, more or leſs perfect fuſion, a different length of time in cooling; and in the latter character I ſuſpect the obſervers to have been deceived by the

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* See Ferber's Travels in Italy, pp. 231, 232. Engliſh tranſlation.

† Dolomieu, Iſles Ponces, and Laves de l'Etna, paſſim.

decay of the rocks they inſpected[*]. At all events, lavas in abundance ſhew, that fire is capable of producing any required degree of compactneſs [†].

I ſhall conclude this induction of particulars with an obſervation lately publiſhed by one of our moſt intelligent mineralogical travellers. "Among the ancient black ſtones, the "compound ſpecies are moſt frequent. They often conſiſt of "*a kind of granite*, in which the ſcaly black ſhoerl predominates "ſo much, that the whole maſs appears black. It is accompa- "nied by white feldſpath of ſo ſmall a grain, and ſo entangled "among the ſhoerl, as to be ſometimes ſcarcely diſtinguiſhable. "The feldſpath itſelf is ſometimes tranſparent, and by tranſ- "mitting the colour of the ſhoerl, in which it is imbedded "(empâté) appears black . . . . . Sometimes ſcales of black "mica occur. The conſtituent parts do not always obſerve "the ſame proportion; and when the quantity of feldſpath "increaſes, the appearance of a real grey or red granite is pro- "duced. Hence we have veins and ſpots of grey granite in "almoſt all the dark-coloured rocks that paſs under the "denomination of baſaltes. Theſe veins have very much "embarraſſed thoſe naturaliſts who maintain that all baſaltes "has been produced by fire." This circumſtance, however, according to my view of the ſubject, is far from embarraſſing: I conſider it as a ſtrong proof of my opinion, ſince it ſeems to involve this conſequence, that, if baſaltes proceed from fuſion, granite alſo muſt. Specimens, ſuch as thoſe here deſcribed, I ſhould place near granular baſaltes, like that of Cape Fairhead. "In blocks of ancient baſaltes," proceeds M. Dolo-

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[*] "Though partial ſpots of this granite are often friable, eſpecially about the ſurface, yet in general it is very hard." Strange, ubi ſupra, p. 9.

[†] See Dolomieu, Iſles Ponces, p. 139.

MIEU, "I have obſerved the tranſition from ſhoerl in a "maſs nearly homogeneous (I ſay, *nearly homogeneous*, be-"cauſe I know of no ſtones, belonging, as theſe do, to the "primitive mountains, without indications of a ſeparation of "ſeveral ſubſtances which were incorporated together in a "paſte, or rather which are generated in that paſte) to black "and white granites, with large grains, and compoſed of "nearly equal quantities of white feldſpath and black ſhoerl. "The tranſition depends altogether on an increaſed proportion "of feldſpath and on the enlargement of its grains; a "phænomenon which leaves no room to doubt, that all theſe "ſtones belong to the ſame ſyſtem of mountains*."

By obſervations like theſe, which the ſpecimens I either poſſeſs, or have examined, corroborate and compleat, I am perſuaded, that when once it becomes an object of attention, perſons who have an opportunity of exploring countries where baſaltes and granite abound, will eaſily find a ſucceſſion of ſpecimens beginning at the former and terminating at the latter. Nor is it perhaps difficult to aſſign highly probable reaſons, why a mixture of different earths with more or leſs of metallic matter, in returning from a ſtate of fuſion to a ſolid conſiſtence, may aſſume ſometimes the homogeneous *baſaltic*, and ſometimes the heterogeneous *granitic* internal ſtructure. No fact is more familiar than that it depends altogether on the management of the fire, and the time of cooling, whether a maſs ſhall have the uniform vitreous fracture, or an earthy broken grain, ariſing from a confuſed cryſtallization. The art of making REAUMUR's porcelain conſiſts entirely in allowing the black glaſs time to cryſtallize by a ſlow refrigeration; and the very ſame maſs, according as the heat is conducted, may, without any alteration of its chemical conſtitution, be ſucceſſively exhibited any number of times as glaſs, or as a ſtony matter with a broken grain. In the ſlag of the iron furnaces, the ſame piece generally exhibits both theſe appearances; the upper ſurface cools faſt, and is glaſs; what lies deeper, loſes its heat more gradually, and is allowed time to take on the cryſtalline arrangement peculiar to its nature, in as far as a number of cryſtals, ſtarting from various points at once, and crowding each other, will admit of it. Here indeed the cryſtals are uniform, and not of a different form and compoſition, as in granite; ſo that this analogy applies cloſely only to baſaltes; and it perfectly explains why this body in congealing has aſſumed an earthy and not a vitreous grain. But it is eaſy to conceive how, under certain variations of heat and mixture, a melted maſs may coagulate into quartz, feldſpath and ſhoerl, or mica*. The moſt permanent difference between baſaltes and granite, as to mixture, conſiſts in the quantity of iron; for the earths in the innumerable varieties of each vary indefinitely in their proportions; and as to heat, that the latter having been perhaps in general raiſed from a greater depth, and conſiſting of more

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* Journal de Phyſique, September 1790, p. 196. I have ſeveral ſpecimens from the whin rocks about Edinburgh perfectly anſwering to this deſcription, and ſo like ſome Veſuvian lavas that it is impoſſible to diſtinguiſh them.

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* "Il eſt certain que dans les porphyres les criſtaux de feld-ſpath n'exiſtoient pas avant l'époque de la précipitation de leur baſe." He ſuppoſes them to be of aqueous origin; but the fact is much to my preſent purpoſe. "On y ſuit les progrès ſucceſſifs de leur formation: on voit que peu à peu les ſubſtances qui leur ſont propres ſe rapprochent, s'epurent et prennent les formes qui conviennent à leurs molécules; ils étoient comme en diſſolution dans leurs matrices et ils ont d'autant plus de facilité a ſe joindre que la fluidité a été plus parfaite et que le deſſèchement a été plus long." Iſles Ponces, 247, 248.

huge

huge maſſes, muſt have cooled more ſlowly, and perhaps they have undergone different degrees of fuſion. Beſides toadſtone, baſaltes incloſing feldſpath, zeolite, &c., various lavas clearly demonſtrate that heterogeneous earthy cryſtals do ſeparate from a fuſed paſte, once undoubtedly as uniform as a metallic calx and its reducing flux before the ſubſidence of the metallic particles. We ſhall, I imagine, be much deceived by a narrow analogy if, becauſe in our proceſſes for glaſs-making an homogeneous product is obtained from heterogeneous materials, we conclude, that an heterogeneous product may not, under other circumſtances, reſult from fuſion; and that fire keeps inſeparably blended whatever it has once reduced to an uniform liquid paſte.

It muſt alſo be carefully remembered, that this difficulty does not preſs the igneous more than the oppoſite hypotheſis. Since the conſtituent parts of granite are cryſtals, the whole maſs muſt once have exiſted in that ſtate of entire diſunion of its particles which is neceſſary to cryſtallization. Now, whether ſuch a ſolution have been effected by the repulſive power of fire, or the intervention of water, it is juſt as eaſy to conceive heterogeneous earthy cryſtals ſhooting from different points of an uniform liquid, according to the former ſuppoſition, as the latter.

In the natural hiſtory of granite and baſaltes, another ſtriking circumſtance occurs: *they lie ſo contiguous, and are ſo involved in one another, that we cannot but ſuppoſe both to have undergone the ſame operations of nature at the ſame time.* This is ſeen with the utmoſt frequency upon every poſſible ſcale, and under a vaſt variety of modifications. The facts already quoted afford inſtances in point. I have before me a ſpecimen from the park at Stockholm, conſiſting partly of trapp and partly of granite. The adjacent parts are as firmly united as the other parts of the ſpecimen; and when a violent blow is ſtruck, the trapp and granite do not ſeparate, but the fracture takes ſome other direction. They ſeem in ſeveral places of the boundary to run into one another. The whole mountainous diſtrict ſurveyed by Mr. Leske [*] with ſuch ſcrupulous accuracy affords multiplied examples of the contiguity and connection between theſe different rocks. “From all theſe minute “deſcriptions,” ſays the Author, “it appears, that the baſe “of the whole range conſiſts of granite. On the declivity of “the higheſt elevations, and on the ſolitary ſummits of the “external chain, *corneous porphyry* lies upon the granite, out “of which as well as the granite itſelf, and the ſandſtone at “its foot, baſaltes has been protruded by the force of ſubter-“raneous fire [†].” The manner of connection will appear from a few examples. The baſaltes of the Spizberg [‡] has a granulated ſtructure, and is imbedded in granite. The ſubſtance of the pillars of the Gikelſberg [§] is cloſe and granular: in ſome pieces “I found the conſtituent grains of granite *little* “*altered*.” Of the columns of the Knorberg, “the ſubſtance “is cloſe, uneven, and conſiſts of diſtinct grains: . . . large “pieces of *imperfectly fuſed* granite are diffuſed through its “ſubſtance. In the Whinſtone of the Hochwald there are “found pieces conſiſting of a mixture of white feldſpath, “quartz, and black ſhoerl [‖].” Again, in the Rauberg, the conſtituent parts of granite are ſo diffuſed through the baſaltes,

[*] Reiſe durch Sachſen.

[†] Ibid. p. 513. “It is alſo remarkable, that granite in general throughout Velay and Auvergne is frequently intermixed with the baſaltine and other common volcanic hills. I have obſerved the ſame in Italy.” Strange, p. 14.

[‡] Reiſe durch Sachſen, p. 328.

[§] Ibid. p. 498.

[‖] Ibid. p. 515.

that the Author imagines the rock to be an imperfectly fuſed granite [*]. I rather conſider theſe as inſtances of imperfectly cryſtallized granite, where ſome unfavourable circumſtance has prevented the conſtituent parts from receding completely from one another. Experiments ſhew, that almoſt all granites melt into a black glaſs [†]; and perhaps it is no abuſe of analogy, nor inconſiſtent with what I have already remarked, to conclude, that granite, in the ſtate of imperfect fuſion, ſhould preſent a glaſſy ſubſtance, involving the more infuſible parts of which this ſtone conſiſts.

The Scheibenberg, near Königſbruck, conſiſts of a ſtone which Mr. LESKE knows not whether to call hornſlate [‡], or corneous porphyry [§]. From the deſcription it appears plainly to be a whinſtone. The colour is dark grey; it breaks into columnar fragments; is hard, fine-grained, and *ſonorous*; little veins of quartz croſs it in all directions, and it frequently becomes porphyritic, as encloſing cryſtals of feldſpath. The Author himſelf is afterwards aware [||] of its affinity to baſaltes, both in ſubſtance and from its aſſuming the columnar form. In this hill a maſs of granite is found imbedded in the whinſtone, and on all ſides ſurrounded by it, and the maſs of granite is in its turn in all directions interſected with veins and ſtripes of whinſtone. Mr. LESKE is much ſtruck by this mutual and intimate incorporation; but he makes no attempt to explain it. In ſome inſtances, he thinks an eruption has broke out through the granite; and in others, is at pains to ſhew, that theſe ſubſtances are not thoroughly blended, as in the laſt example, and in that deſcribed by FERBER.

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[*] Reiſe durch Sachſen, p. 330.

[†] HAIDINGER's Eintheilung der Gebirgsarten. Wien, 1787, p. 11. and the Authors quoted there.

[‡] Hornſchiefer.

[§] Ibid. p. 24—29. No chemical characters are given.

[||] Ibid. p. 513, 514

It may be ſaid, and no doubt it ſometimes happens, that ſhivers of granite, broken off by the violence of exploſion, are licked up by melted matter as it moves along; thus, in volcanic breccias an older lava is incloſed in one more recent, and thus what is called primary is ſometimes encaſed in ſecondary granite. But ſuch an hypotheſis is too narrow to embrace all the phænomena. It does not explain the incipient coagulation of the uniform paſte into grains, and thoſe the different grains of granite; nor the diffuſion of the conſtituent parts of granite through the ſubſtance of baſaltes; nor the fifth ſpecies deſcribed by Mr. FERBER.

In the whinſtone rocks of England, which are far more numerous than is commonly ſuppoſed, I have frequently obſerved in the ſame hill, 1. homogeneous dark grey ſtone; 2. feldſpath encloſed in this as in a paſte; and, 3. the paſte diſappearing, and the whole becoming granular, and the grains heterogeneous. Beſides feldſpath, quartz is found in innumerable maſſes of varying magnitude * in many whinſtone rocks, and as proper baſaltes is but a confuſed maſs of cryſtals of ſhoerl, we have all the ingredients of granite; and why

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* In the Wrekin, Cader Idris, &c. numerous and large veins filled with quartz occur; but theſe have not been *ſecreted* from the ſubſtance of the rocks in which they lie. They, perhaps, ſhew this moſt ſiliceous ingredient of granite to have been near and in fuſion at the ſame time. About Cader Idris I ſuſpect an incorporation of granite and baſaltes may be found. I have ſeen pieces of granite about that mountain which did not ſeem to have been far removed from the rock to which they belonged; but I had then no particular inducement to make an accurate examination.

may we not expect to find them incorporated together, and in every ſtate of diffuſion and ſeparation?

Under this head I ſhall only remark further, that ſeveral late obſervations, from which it has been inferred, that certain extinct volcanos have been ſeated in the heart of granite *, ſeem to admit of a much more eaſy explanation, on the ſuppoſition that granite has cryſtallized from fuſion. 1. Volcanic fires reach to a much greater depth than any at which we have had an opportunity of making obſervations. The focus in different inſtances may be ſeated at a different diſtance from the ſurface; but none are probably leſs than ſeveral miles at leaſt deep. 2. The currents of granitic lava † in the Pontian iſles leave little room to doubt of the power of ſubterraneous fire to produce this ſubſtance. To ſuppoſe them to be rocks of granite fuſed, but otherwiſe unchanged, and that even fiſſile rocks may be made to flow without loſing their laminated ſtructure; is as bold an aſſumption as can eaſily be taken up. In the great igneous proceſſes of nature fire need not be imagined to act otherwiſe ‡

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* DOLOMIEU, Iſles Ponces, p. 30, 31. and Iſles de Lipari, in various parts.

† DOLOMIEU, *l. c. paſſim*, and particularly p. 89—97. Inſtructive as the particular facts deſcribed by this excellent mineralogiſt always are, I muſt diſſent from him both on the mode of action of volcanic fires, and on the production of zeolite and other cryſtals, in glandular rocks, for the reaſons aſſigned here and below.

‡ "Le feu produit, dans les laves, une fluidité qui n'a aucun rapport avec la fluidité vitreuſe que nous opérons . . . . . Celui des volcans n'a point d'intenſité; il ne peut pas même vitrifier les ſubſtances les plus fuſibles . . . . il produit la fluidité par une eſpece de diſſolution, par une ſimple dilatation qui permet aux parties de gliſſer les unes ſur les autres (DOLOMIEU, Avant-propos, p. 8.). See alſo p. 84. Fire in a crucible produces fluidity no otherwiſe; and when there is this freedom of motion among the *particles*, how can we ſuppoſe the cryſtals of granite and the leaves of ſchiſtus to remain unmixed even in a current of lava?

than in our ſmall experiments; we actually ſee it producing glaſs and cellular ſpongy ſcoriæ: when the products are of a different character, we muſt have recourſe to acceſſory circumſtances, and not violate the plaineſt rules of philoſophiſing by attributing different effects to the ſame cauſe. The latent motive for ſuch an extraordinary hypotheſis may eaſily be divined; the obſerver took it for granted, that all granite is of aqueous formation; hence he was obliged to reaſon backwards from the unknown, that of the Alps for inſtance, to the known, inſtead of proceeding from the palpable effects of ſubterraneous fire by eaſy ſteps to a general theory of granite. When it is taken for granted, before examination, that granite cannot be formed by fire, there remains no reſource but to ſay, that granitic * lavas are granite rocks fuſed, but not altered in the arrangement of their conſtituent parts. 3. Though the heat of volcanos be ſometimes and in ſome places moderate; in others we have good reaſon to believe, that it exceeds any degree we can produce, except by means of factitious air; we are certain that it forms molten currents of petroſilex and flint exactly the ſame as our gun flints †.

If we admit this reaſoning, the appearance of granite in the boſom of volcanic deſolation may, if duly examined in all its circumſtances, afford ſtrong evidence of its production by fuſion; and it is reaſonable to conclude, that it was once covered to a conſiderable depth by erupted matters, which the courſe of

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* "Les laves blanches de l'Ile Ponce paroiſſent appartenir plus particulièrement au granit et aux roches feuilletées granitiques. On reconnoît les ſubſtances qui conſtituent ordinairement ce genre de roche compoſée dans preſques toutes les matières blanches (volcaniques) de cette iſle; ſavoir, le quartz en grain, le mica noir écailleux et le feldſpath plus ou moins pur. Dolomieu, *l. c.* p. 89.

† Idem ibid. p. 107.

time, and the injuries of the atmoſphere, have removed; though I by no means deny that a volcano may force its way through pre-exiſting rocks of granite.

There is ſtill another analogy between baſaltes and granite, more important to the theory of the earth, and leſs liable to controverſy than either of the preceding. *In their ſituation, with reſpect to other rocks, we may obſerve the ſame law.* The general rule of ſuper-poſition, reckoning from below upwards, is, 1. granite; 2. ſchiſtus; 3. limeſtone. This rule has been found to hold good by ſo many mineralogical travellers that, though it may not be abſolutely univerſal, it muſt be allowed to prevail very extenſively. Now, in this iſland there are numerous inſtances where baſaltes is ſubſtituted in the ſeries inſtead of granite, and where it ſeems to alternate with granite as the ſubſtratum of other rocks. On the road from Dolgelly in Merionethſhire, by Mallwhyd and Cann's Office, through Llanfair to Welchpool, ſchiſtus appears always incumbent on whinſtone, except ſometimes when the latter is interjected between the ſtrata, or ſqueezed up through fiſſures. In Wales the country is ſo hilly, that the limeſtone, if it exiſted, has probably been waſhed away; but on the confines of England it comes in. The road from Welchpool to Shrewſbury paſſes over the ſide of the Long Mountain, which conſiſts of ſchiſtus; on the left, or towards the eaſt, riſe ſome conſiderable baſaltic hills. The ſtrata of the Long Mountain point towards the ſummit of theſe hills, as if the narrow valley that intervenes had been cut by water upon the lifted edge of the ſchiſtus. At a ſmall diſtance from the north and ſouth ſides of the baſaltic hills calcareous ſtrata are found. Beyond Shrewſbury, on the road to London, we have, inſtead of the continued ridges of Wales, a number of inſulated, and generally rugged, points, riſing over the face of Shropſhire and the adjacent counties. Were the plains covered with water a few yards in depth, theſe eminences would appear from diſtance to diſtance like ſo many ſtepping ſtones. They all, except the Malvern Hills, which, though compoſed of granite, I conſider as part of the ſame ſyſtem, conſiſt of whinſtone. Among theſe ſtepping ſtones I reckon the baſaltic hills near Welſhpool, the Wrekin, Lilleſhall Hill, and, at a greater diſtance towards the Eaſt, the riſing grounds near Newcaſtle in Staffordſhire, whence the whin rock, perhaps, communicates by the toadſtone of Derbyſhire, through the hills in the North of England with the whinſtone towards the South of Scotland. In a ſouth or ſouth-weſt direction from the Wrekin, a number of craggy eminences ariſe. They are baſaltes, and form a ſtriking contraſt with the ſmooth, rounded, and lumpiſh ſwells of ſchiſtus in their neighbourhood. From the whin rocks near Stretton we may paſs by the Brown and Titterſtone Clee Hills (on the latter of which are regular priſmatic columns) to the Malvern Hills. About theſe hills lie ſtrata of ſchiſtus and limeſtone, as is ſeen on the road from Much Wenlock to Stretton. To the ſouth-eaſt an extenſive field of whinſtone, with occaſional elevations, is ſpread over the confines of Worceſterſhire, Warwickſhire, and Staffordſhire. Here we have the Rowley ragſtone. Whether the baſaltes proceeds ſouthward by ſuch interruptions till it join the Elvin or whinſtone, and granite of Devonſhire and Cornwall, where I imagine they may be found incorporated, I wiſh for an opportunity to examine. In the plain part of this whole diſtrict, the whin rock appears frequently at the ſurface, or a little below the ſtrata, ſo that the hills have probably a ſubterraneous communication with one another, and there needed but a little more lifting force to form continued ranges of mountains. The road from Welſhpool to Birmingham, above threeſcore miles, is repaired in a great meaſure with whinſtone. A colonnade of baſaltes has been lately expoſed in digging the Shropſhire canal; and in the mining country around, levels have been driven in the black rock, as it is ſometimes called. As I have ſeen whinſtone and ſlate in various other parts of North and South Wales, the whole weſtern ſide of our iſland has probably been raiſed by the baſaltes on which the ſuperficial ſtrata now reſt, though from particular circumſtances the fuſed maſs has now and then cryſtallized into granite; and as it has been conjectured, that the baſaltes of Ireland once joined that of the Scotch iſles and the main land itſelf, ſo perhaps the baſaltes of North Wales joined the Iriſh coaſt till the ſea worked its way or broke in, and deſtroyed the continuation.

As limeſtone is ſometimes ſaid to reſt immediately on granite [*], ſo at the foot of the Wrekin, and at Lilleſhall Hill, no ſlate is interpoſed between the limeſtone and baſaltes; ſo that the analogy extends even to the exceptions.

But another ſeries has been obſerved, which ſeems to connect granite by a cloſer tie with the operations of ſubterraneous fire. In Italy lava ſtands to ſlate and limeſtone in the ſame relation as granite and whinſtone in other countries [†]. Whole ridges of mountains in the Venetian territory conſiſt of ſolid lava, ſometimes almoſt bare, ſometimes retaining the ſuperincumbent ſtrata, with ſeveral local variations; all of which are reducible to a greater or leſs degree of lifting force. Theſe chains have a totally different form from the common conical ſhape of volcanos or heaps of looſe ejected matters. They ſeem to afford a clear inſtance of the manner in which long continuations of mountains have been elevated; for it is not eaſy to admit the ſuppoſition of the obſerver, who has ſo accurately deſcribed them, that the limeſtone has been converted into lava; and that the ridges exiſted, ſuch as they appear at this day, before this change was produced by ſubterraneous ſire. Chemical and mechanical conſiderations are unfavourable to this hypotheſis; and "ſince moſt of theſe branches, whether "marine, volcanic, or mixed, preſerve nearly the ſame exter-"nal characters, directions, and paralleliſm;" it appears highly probable, that they have not pre-exiſted as hills in another ſtate, but owe their elevation to the expanſive force of fire; and that the ſame lava which appears in ſo many places lies alſo under all the limeſtone hills, of which indeed there are evident indications.

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[*] BORN's Letters, p. 207.

[†] FERBER, *l. c.* pp. 42. 51.; and eſpecially STRANGE, *l. c.* p. 24—32.

Several modern travellers have deſcribed the ſtrata of granite mountains; but neither in their deſcriptions nor drawings have I been able to find ſatisfactory evidence of this arrangement; nor have I obſerved it in nature. A liquid maſs ſwelled by heat muſt crack in cooling. Granite ſeems to me to have cracked moſt frequently like the *baſalte en tables*; and theſe flat maſſes have been taken for ſtrata. A ſtratum, conſiſting of proper materials to form whinſtone or granite, may have been expoſed to the neceſſary degree of heat, and poſſibly have undergone this change without much relative local derangement. Should ſuch a ſtratum be diſcovered, it would afford no proof of the ſtratification of the great mountains of granite or ſhapeleſs whinſtone, which, in conſequence of its numerous fiſſures in all directions, ſometimes aſſumes enough of this appearance to impoſe on an unwary eye.

One conſequence of theſe obſervations is too important to be omitted. They lead us to reject the common diviſion of mountains into primary and ſecondary. The chains of granite, ſchiſtus, and limeſtone, muſt be all coeval; for if the central chain of the Alps burſt as a body expanded by heat from the bowels of the earth, it reared the bordering chains at the ſame effort. But it muſt be recollected, that the mountains no longer wear their original form, vallies having been cut between and through them, and various other effects of dilapidation having taken place. It is by no means difficult to underſtand why no exuviæ of organized bodies are found in theſe imaginary primitive mountains. Riſing from a great depth, they threw aſide the ſuperficial accumulations of the ancient ocean. What was deepeſt is therefore now moſt central; and what lay on the ſurface now ſkirts the high interior chains. Hence the ſtrata reſt indifferently on granite, baſaltes, or lava; all which ſubſtances derive from their ſituation an equal claim to be regarded as primordial materials. It is a little ſurprizing, that this inveterate error, which has effectually barred the way to all great diſcoveries in geology till of late *, ſhould have prevailed ſo long: for, 1. it is well known, that granite is ſometimes found encloſing pieces of ſchiſtus †; nor are long ſtretches of ſlate uncommon in mountains of granite ‡. Now, how can a ſecondary be ſo enveloped in a primitive rock? and how eaſy is this to be underſtood, if we ſuppoſe granite as a fuſed maſs raiſing, rending, and ſhivering the incumbent ſtrata, while its heat hardened them into laminated ſtone. 2. Suppoſing granite mountains previouſly exiſting in the ancient ocean, the inclination of the incumbent ſtrata, and their diſarrangement is ſuch that they could never have been depoſited as they appear at preſent; they would have been much more horizontal in their direction. It ſeems impoſſible to attribute the diſorderly deviation, which is ſo general in the mountains of ſlate, &c. from that poſition which all ſediments from water aſſume, to any thing but a force lifting from below, and ſometimes burſting through.

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* Till Dr. HUTTON's Theory of the Earth, Edinb. Tranſ. Vol. I. appeared.

† BORN's Letters, p. 207, 208.

‡ HAIDINGER, *l. c.* p. 18.

It is moreover certain, that all theſe lifting maſſes, from granite to acknowledged lava [*], are found ſqueezed up through fiſſures formed in the ſtrata by their own expanſion. This, and not the infiltration of water, as M. DE SAUSSURE would perſuade us [†], appears to be the true origin of ſuch veins of granite.

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[*] FERBER, *l. c.* p. 51.

[†] Voyages dans les Alpes, 4to, I. 532—536. The whole paſſage well deſerves the notice of thoſe who are intereſted in theſe enquiries. Vertical ſtrata, in one inſtance, lying againſt the foot of a granite mountain, are divided by oblique fiſſures, full of granite. This naturaliſt ſuppoſes them to have filled up gradually by the rain water diſſolving particles of granite, carrying them down and depoſiting them in the form of granite again; two operations which one may ſafely deny to rain water the power of performing. Other inſuperable objections to this theory ſtart up at every ſtep in the deſcription. If water can diſſolve any, it is ſurely but a very ſmall part of all the ingredients of granite. Now, ſuppoſe a fiſſure full of ſuch a ſolution; the water is, I ſuppoſe, to evaporate, and the cryſtallization to take place; the cryſtals muſt be ſmall, on account of the ſmall quantity of matter to form them, and a ſucceeding ſolution can only yield another crop of ſmall cryſtals, it will not enlarge thoſe already formed; but we are told, that the cryſtals of granite in the crevices in queſtion are remarkably large. This has always appeared to me a chemical demonſtration of the falſehood of a very common ſuppoſition, that the cryſtals often occurring in the cavities of

the amygdaloides rocks have been formed by the infiltration of water. The cryſtals are frequently of ſo large a ſize that you can by no means ſuppoſe the quantity of water, at any one time exiſting in the cavity, could have held the ſolid matter, of which they conſiſt, in ſolution. Now, I think, it is contrary to all our experience in chemiſtry, to ſuppoſe cryſtals built up by ſucceſſive operations. If upon cryſtals of nitre I pour a ſolution of the ſame ſalt, the former cryſtals will not be enlarged and amended, but a new ſet will be formed; ſo ſucceſſive quantities of water, paſſing through theſe cavities, ought to form ſucceſſive ſets of very ſmall cryſtals. Neither can I imagine, what cauſes can produce within theſe cavities a depoſition of the matter once diſſolved by the water. It is not cooled; it does not evaporate; it loſes no fixed air; it comes in contact with no new matter, whoſe attraction may overpower the attraction of the water.

The diviſions, "rather marked out than formed, which croſs each other irregularly (p. 533.), and indicate an incipient retraction," are much more conſiſtent with a ſimultaneous congelation than a gradual appoſition; and the granite is the ſame as that of the contiguous hill in the colour and appearance of its conſtituent parts. This coincidence is a ſtriking fact.

In truth, the philoſophers who attribute the formation of granite to water ſeem not to have advanced a ſtep in their proof beyond the equivocal circumſtance of its being a cryſtallized maſs.

IV. *On Nebulous Stars, properly ſo called.*

By William Herſchel, *LL.D. F. R. S.*

Read February 10, 1791.

IN one of my late examinations of a ſpace in the heavens, which I had not reviewed before, I diſcovered *a ſtar of about the 8th magnitude, ſurrounded with a faintly luminous atmoſphere, of a conſiderable extent.* The phænomenon was ſo ſtriking that I could not help reflecting upon the circumſtances that attended it, which appeared to me to be of a very inſtructive nature, and ſuch as may lead to inferences which will throw a conſiderable light on ſome points relating to the conſtruction of the heavens.

Cloudy or nebulous ſtars have been mentioned by ſeveral aſtronomers; but this name ought not to be applied to the objects which they have pointed out as ſuch; for, on examination, they proved to be either mere cluſters of ſtars, plainly to be diſtinguiſhed with my large inſtruments, or ſuch nebulous appearances as might be reaſonably ſuppoſed to be occaſioned by a multitude of ſtars at a vaſt diſtance. The milky way itſelf, as I have ſhewn in ſome former Papers, conſiſts intirely of ſtars, and by imperceptible degrees I have been led on from the moſt evident congeries of ſtars to other groups in which the lucid points were ſmaller, but ſtill very plainly to be ſeen; and from them to ſuch wherein they could but barely be ſuſpected, till I arrived at laſt to ſpots in which no trace of a ſtar a ſtar was to be diſcerned. But then the gradations to theſe latter were by ſuch well-connected ſteps as left no room for doubt but that all theſe phænomena were equally occaſioned by ſtars, variouſly diſperſed in the immenſe expanſe of the univerſe.

When I purſued theſe reſearches, I was in the ſituation of a natural philoſopher who follows the various ſpecies of animals and inſects from the height of their perfection down to the loweſt ebb of life; when, arriving at the vegetable kingdom, he can ſcarcely point out to us the preciſe boundary where the animal ceaſes and the plant begins; and may even go ſo far as to ſuſpect them not to be eſſentially different. But recollecting himſelf, he compares, for inſtance, one of the human ſpecies to a tree, and all doubt upon the ſubject vaniſhes before him. In the ſame manner we paſs through gentle ſteps from a coarſe cluſter of ſtars, ſuch as the Pleiades, the Præſepe, the milky way, the cluſter in the Crab, the nebula in Hercules, that near the preceding hip of Bootes (*a*), the 17th, 38th, 41ſt of the 7th claſs of my Catalogues (*b*), the 10th, 20th, 35th of the 6th claſs (*c*), the 33d, 48th, 213th of the 1ſt (*d*), the 12th,

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(*a*) RA. 13 h. 27′ 40″. PD. 60° 2′. The places of all the objects mentioned in this Paper are not brought to the preſent time, but given as they were calculated from the beſt obſervations I have made of them; the change in their ſituation ariſing from the lapſe of a few years is too trifling to be any hindrance to our finding them very eaſily.

				h.	′	″		°	′
(*b*)	VII.	17.	RA.	7	9	45.	PD.	114	34.
		38.		6	53	16.		88	37.
		41.		22	20	20.		38	47.
(*c*)	VI.	10.		16	14	22.		115	32.
		20.		0	42	4.		117	46.
		35.		0	19	44.		29	41.
(*d*)	I.	33.		11	57	26.		78	25.
		48.		17	10	46.		107	36.
		213.		12	17	59.		44	45.

150th, 756th of the 2d (*a*), and the 18th, 140th, 725th of the 3d (*b*), without any heſitation, till we find ourſelves brought to an object ſuch as the nebula in Orion, where we are ſtill inclined to remain in the once adopted idea, of ſtars exceedingly remote, and inconceivably crowded, as being the occaſion of that remarkable appearance. It ſeems, therefore, to require a more diſſimilar object to ſet us right again. A glance like that of the naturaliſt, who caſts his eye from the perfect animal to the perfect vegetable, is wanting to remove the veil from the mind of the aſtronomer. The object I have mentioned above, is the phænomenon that was wanting for this purpoſe. View, for inſtance, the 19th cluſter of my 6th claſs (*c*), and afterwards caſt your eye on this cloudy ſtar (*d*), and the reſult will be no leſs deciſive than that of the naturaliſt we have alluded to. Our judgement, I may venture to ſay, will be, that *the nebuloſity about the ſtar is not of a ſtarry nature.*

But, that we may not be too precipitate in theſe new deciſions, let us enter more at large into the various grounds which induced us formerly to ſurmiſe, that every viſible object, in the extended and diſtant heavens, was of the ſtarry kind, and collate them with thoſe which now offer themſelves for the contrary opinion.

				h.	′	″		°	″
(*a*)	II.	12.	RA.	12	32	37.	PD.	72	23.
		150.		14	19	53.		81	43.
		756.		14	51	42.		35	22.
(*b*)	III.	18.		12	41	7.		84	51.
		140.		1	5	8.		92	0.
		725.		12	6	57.		43	14.
(*c*)	VI.	19.		15	5	2.		110	14.
(*d*)				3	56	48.		59	50.

It has been obſerved, on a former occaſion, that all the ſmaller parts of other great ſyſtems, ſuch as the planets, their rings and ſatellites, the comets, and ſuch other bodies of the like nature as may belong to them, can never be perceived by us, on account of the faintneſs of light reflected from ſmall, opaque objects; in my preſent remarks, therefore, all theſe are to be intirely ſet aſide.

A well connected ſeries of objects, ſuch as we have mentioned above, has led us to infer, that all nebulæ conſiſt of ſtars. This being admitted, we were authorized to extend our analogical way of reaſoning a little farther. Many of the nebulæ had no other appearance than that whitiſh cloudineſs, on the blue ground upon which they ſeemed to be projected; and why the ſame cauſe ſhould not be aſſigned to explain the moſt extenſive nebuloſities, as well as thoſe that amounted only to a few minutes of a degree in ſize, did not appear. It could not be inconſiſtent to call up a teleſcopic milky way, at an immenſe diſtance, to account for ſuch phænomena; and if any part of the nebuloſity ſeemed detached from the reſt, or contained a viſible ſtar or two, the probability of ſeeing a few near ſtars, apparently ſcattered over the far diſtant regions of myriads of ſidereal collections, rendered nebulous by their diſtance, would alſo clear up theſe ſingularities.

In order to be more eaſily underſtood in my remarks on the comparative diſpoſition of the heavenly bodies, I ſhall mention ſome of the particulars which introduced the ideas of *connection* and *diſjunction:* for theſe, being properly founded upon an examination of objects that may be reviewed at any time, will be of conſiderable importance to the validity of what we may advance with regard to my lately diſcovered nebulous ſtars.

On

On June the 27th, 1786, I ſaw a beautiful cluſter of very ſmall ſtars of various ſizes, about 15′ in diameter, and very rich of ſtars (*a*). On viewing this object, it is impoſſible to withhold our aſſent to the idea which occurs, that theſe ſtars are connected ſo far one with another as to be gathered together, within a certain ſpace, of little extent, when compared to the vaſt expanſe of the heavens. As this phænomenon has been repeatedly ſeen in a thouſand caſes, I may juſtly lay great ſtreſs on the idea of ſuch ſtars being connected.

In the year 1779, the 9th of September, I diſcovered a very ſmall ſtar near ε Bootis (*b*). The queſtion here occurring, whether it had any connection with ε or not, was determined in the negative; for, conſidering the number of ſtars ſcattered in a variety of places, it is very far from being uncommon, that a ſtar at a great diſtance ſhould happen to be nearly in a line drawn from the ſun through ε, and thus conſtitute the obſerved double ſtar.

The 7th of September, 1782, when I firſt ſaw the planetary nebula near ν Aquarii (*c*), I pronounced it to be a ſyſtem whoſe parts were connected together. Without entering into any kind of calculation, it is evident, that a certain equal degree of light within a very ſmall ſpace, joined to the particular ſhape this object preſents to us, which is nearly round, and even in its deviation conſiſtent with regularity, being a little elliptical, ought naturally to give us the idea of a conjunction in the things that produce it. And a conſiderable addition to this argument may be derived from a repetition of the ſame phænomenon, in nine or ten more of a ſimilar conſtruction.

(*a*) RA. 18 h. 20′ 2″. PD. 107° 3′.

(*b*) Phil. Tranſ. Vol. LXXII. p. 115. Catalogue of Double Stars, I. 1.

(*c*) RA. 20 h. 52′ 36″. PD. 102° 12′.

When I examined the cluſter of ſtars, following the head of the great dog (*a*), I found on the 19th of March, 1786, that there was within this cluſter a round, reſolvable nebula, of about two minutes in diameter, and nearly of an equal degree of light throughout (*b*). Here, conſidering that the cluſter was free from nebuloſity in other parts, and that many ſuch cluſters, as well as many ſuch nebulæ, exiſt in divers parts of the heavens, it appeared to me very probable, that the nebula was unconnected with the cluſter; and that a ſimilar reaſon would as eaſily account for this appearance as it had reſolved the phænomenon of the double ſtar near ε Bootis; that is, a caſual ſituation of our ſun and the two other objects nearly in a line. And though it may be rather more remarkable, that this ſhould happen with two compound ſyſtems, which are not by far ſo numerous as ſingle ſtars, we have, to make up for this ſingularity, a much larger ſpace in which it may take place, the cluſter being of a very conſiderable extent.

On the 15th of February, 1786, I diſcovered that one of my planetary nebulæ (*c*), had a ſpot in the center, which was more luminous than the reſt, and with long attention, a very bright, round, well defined center became viſible. I remained not a ſingle moment in doubt, but that the bright center was connected with the reſt of the apparent diſk.

In the year 1785, the 6th of October, I found a very bright, round nebula, of about 1½ minute in diameter (*d*). It has a large, bright nucleus in the middle, which is undoubtedly

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		h.	′	″		°	′
(*a*)	RA.	7	32	1.	PD.	104	18.
(*b*)		7	32	5.		104	15.
(*c*)		17	58	25.		23	22.
(*d*)		3	30	35.		109	15.

connected with the luminous parts about it. And though we muſt confeſs, that if this phænomenon, and many more of the ſame nature, recorded in my catalogues of nebulæ, conſiſt of cluſtering ſtars, we find ourſelves involved in ſome difficulty to account for the extraordinary condenſation of them about the center; yet the idea of a connection between the outward parts and theſe very condenſed ones within is by no means leſſened on that account.

There is a teleſcopic milky way, which I have traced out in the heavens in many ſweeps made from the year 1783 to 1789 (*a*). It takes up a ſpace of more than 60 ſquare degrees of the heavens, and there are thouſands of ſtars ſcattered over it: among others, four that form a trapezium, and are ſituated in the well known nebula of Orion, which is included in the above extent. All theſe ſtars, as well as the four I have mentioned, I take to be intirely unconnected with the nebuloſity which involves them in appearance. Among them is alſo *d* Orionis, a cloudy ſtar, improperly ſo called by former aſtronomers; but it does not ſeem to be connected with the milkineſs any more than the reſt.

I come now to ſome other phænomena, that, from their ſingularity, merit undoubtedly a very full diſcuſſion. Among the reaſons which induced us to embrace the opinion, that all very faint milky nebuloſity ought to be aſcribed to an aſſemblage of ſtars is, that we could not eaſily aſſign any other cauſe of ſufficient importance for ſuch luminous appearances, to reach us at the immenſe diſtance we muſt ſuppoſe ourſelves to be from them. But if an argument of conſiderable force ſhould now be brought forward, to ſhew the exiſtence of a luminous matter, in a ſtate of modification very different from the con-

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(*a*) RA. from 5 h. 15′ 8″ to 5 h. 39′ 1′. PD. from 87° 46′ to 98° 10′.

ſtruction of a ſun or ſtar, all objections, drawn from our incapacity of accounting for new phænomena upon old principles, will loſe their validity.

Hitherto I have been ſhewing, by various inſtances in objects whoſe places are given, in what manner we may form the ideas of connection and its contrary by an attentive inſpection of them only: I will now relate a ſeries of obſervations, with remarks upon them as they are delivered, from which I ſhall afterwards draw a few ſimple concluſions, that ſeem to be of conſiderable importance.

To diſtinguiſh the obſervations from the remarks, the former are given in italics, and the date annexed is that on which the objects were diſcovered; but the deſcriptions are extracted from all the obſervations that have been made upon them.

October 16, 1784. *A ſtar of about the 9th magnitude, ſurrounded by a milky nebuloſity, or chevelure, of about 3 minutes in diameter. The nebuloſity is very faint, and a little extended or elliptical, the extent being not far from the meridian, or a little from north preceding to ſouth following. The chevelure involves a ſmall ſtar, which is about 1½ minute north of the cloudy ſtar; other ſtars of equal magnitude are perfectly free from this appearance* (*a*).

My preſent judgement concerning this remarkable object is, that the nebuloſity belongs to the ſtar which is ſituated in its center. The ſmall one, on the contrary, which is mentioned as involved, being one of many that are profuſely ſcattered over this rich neighbourhood, I ſuppoſe to be quite unconnected with this phænomenon. A circle of three minutes in diameter is ſufficiently large to admit another ſmall ſtar, without any bias to the judgement I form concerning the one in queſtion.

(*a*) RA. 5 h. 57′ 4″. PD. 96° 22′.

It muſt appear ſingular, that ſuch an object ſhould not have immediately ſuggeſted all the remarks contained in this Paper; but about things that appear new we ought not to form opinions too haſtily, and my obſervations on the conſtruction of the heavens were then but entered upon. In this caſe, therefore, it was the ſafeſt way to lay down a rule not to reaſon upon the phænomena that might offer themſelves, till I ſhould be in poſſeſſion of a ſufficient ſtock of materials to guide my reſearches.

October 16, 1784. *A ſmall ſtar of about the 11th or 12th magnitude, very faintly affected with milky nebuloſity; other ſtars of the ſame magnitude are perfectly free from this appearance.* Another obſervation mentions 5 *or* 6 *ſmall ſtars within the ſpace of* 3 *or* 4′, *all very faintly affected in the ſame manner, and the nebuloſity ſuſpected to be a little ſtronger about each ſtar.* But a third obſervation rather oppoſes this increaſe of the faintly luminous appearance (*a*).

Here the connection between the ſtars and the nebuloſity is not ſo evident as to amount to conviction; for which reaſon we ſhall paſs on to the next.

January the 6th, 1785. *A bright ſtar with a conſiderable milky chevelure; a little extended,* 4 *or* 5′ *in length, and near* 4′ *broad; it loſes itſelf inſenſibly. Other ſtars of equal magnitude are perfectly free from this chevelure* (*b*).

The connection between the ſtar and the chevelure cannot be doubted, from the inſenſible gradation of its luminous appearance, decreaſing as it receded from the center.

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		h.	′	″		°	′
(*a*)	RA.	6	0	33.	PD.	96	13.
(*b*)		5	30	53.		92	21.

January 31, 1785. *A pretty conſiderable ſtar, with a very faint, and very ſmall, irregular, milky chevelure; other ſtars of the ſame ſize are perfectly free from ſuch appearance* (*a*).

I can have no doubt of the connection between the ſtar and its chevelure.

October 5, 1785. *A ſtar with a ſtrong bur all around.* A ſecond obſervation calls it *a very bright nucleus, with a milky nebuloſity, of no great extent.* A third ſuſpects *the milkineſs to belong to more of the ſame, which is diffuſed over the whole ſweep in that place*; but a fourth ſays, that *the milky nebuloſity is much ſtronger than what the nebulous ground, on which the ſtar is placed, intitles it to* (*b*).

The connection, therefore, between the nebuloſity and the ſtar is evident.

January 1, 1786. *A ſtar ſurrounded with milky chevelure; the ſtar is not central.* A ſecond obſervation calls it *affected with a very faint, and extenſive, milky chevelure.* A third only mentions *a ſtar affected with milky chevelure* (*c*).

As by the word chevelure I always denoted ſomething relating to a center, the connection cannot be doubted.

February 24, 1786. *A conſiderable ſtar, very faintly affected with milky chevelure.* A ſecond obſervation, much the ſame (*d*).

November 28, 1786. *A ſtar involved in milky chevelure* (*e*).

		h.	′	″		°	′
(*a*)	RA.	6	54	27.	PD.	100	53.
(*b*)		5	25	57.		96	52.
(*c*)		5	35	56.		89	50.
(*d*)		5	59	4.		96	19.
(*e*)		5	57	4.		96	15.

January 17, 1787. *A ſtar with a pretty ſtrong milky nebuloſity, equally diſperſed all around; the ſtar is of about the 9th magnitude.* A memorandum to the obſervation ſays, that, having but juſt begun, *I ſuſpected the glaſs to be covered with damp, or the eye out of order; but yet a ſtar of the 10th or 11th magnitude, juſt north of it, was free from the ſame appearance.* A ſecond obſervation calls it *one of the moſt remarkable phænomena I ever have ſeen, and like my northern planetary nebula in its growing ſtate* (*a*).

The connection between the ſtar and the milky nebuloſity is without all doubt.

November 3, 1787. *A bright ſtar with faint nebuloſity.* A ſecond obſervation mentions *the ſtar to be of the 9th magnitude, and the faint nebuloſity of very little extent* (*b*).

June 11, 1787. *Suſpected, ſtellar.* By a ſecond obſervation it is verified, and called *a very ſmall ſtar involved in extremely faint nebuloſity* (*c*).

November 25, 1788. *A ſtar of about the 9th magnitude, ſurrounded with very faint milky nebuloſity; other ſtars of the ſame ſize are perfectly free from that appearance. Leſs than 1′ in diameter. The ſtar is either not round or double* (*d*).

March 23, 1789. *A bright, conſiderably well defined nucleus, with a very faint, ſmall, round chevelure* (*e*).

The connection admits of no doubt; but the object is not perhaps of the ſame nature with thoſe which I call cloudy ſtars.

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		h.	′	′		°	′
(*a*)	RA.	7	16	28.	PD.	68	39.
(*b*)		23	11	26.		30	0.
(*c*)		17	1	51.		47	26.
(*d*)		0	1	57.		18	41.
(*e*)		11	12	25.		50	17.

April 14, 1789. *A conſiderable, bright, round nebula; having a large place in the middle of nearly an equal brightneſs, but leſs bright towards the margin* (*a*).

This ſeems rather to approach to the planetary ſort.

March 5, 1790. *A pretty conſiderable ſtar of the 9th or 10th magnitude, viſibly affected with very faint nebuloſity of little extent, all around. A power of 300 ſhewed the nebuloſity of greater extent* (*b*).

The connection is not to be doubted.

March 19, 1790. *A very bright nucleus, with a ſmall, very faint chevelure, exactly round. In a low ſituation, where the chevelure could hardly be ſeen, this object would put on the appearance of an ill-defined, planetary nebula, of 6, 8, or 10″ diameter* (*c*).

November 13, 1790. *A moſt ſingular phænomenon! A ſtar of about the 8th magnitude, with a faint luminous atmoſphere, of a circular form, and of about 3′ in diameter. The ſtar is perfectly in the center, and the atmoſphere is ſo diluted, faint, and equal throughout, that there can be no ſurmiſe of its conſiſting of ſtars; nor can there be a doubt of the evident connection between the atmoſphere and the ſtar. Another ſtar not much leſs in brightneſs, and in the ſame field with the above, was perfectly free from any ſuch appearance* (*d*).

This laſt object is ſo deciſive in every particular, that we need not heſitate to admit it as a pattern, from which we are authorized to draw the following important conſequences.

		h.	′	″		°	′
(*a*)	RA.	11	45	12.	PD.	33	43.
(*b*)		6	58	40.		91	29.
(*c*)		9	27	22.		30	11.
(*d*)		3	56	48.		59	50.

Suppoſing the connection between the ſtar and its ſurrounding nebuloſity to be allowed, we argue, that one of the two following caſes muſt neceſſarily be admitted. In the firſt place, if the nebuloſity conſiſt of ſtars that are very remote, which appear nebulous on account of the ſmall angles their mutual diſtances ſubtend at the eye, whereby they will not only, as it were, run into one another, but alſo appear extremely faint and diluted; then, what muſt be the enormous ſize of the central point, which outſhines all the reſt in ſo ſuperlative a degree as to admit of no compariſon? In the next place, if the ſtar be no bigger than common, how very ſmall and compreſſed muſt be thoſe other luminous points that are the occaſion of the nebuloſity which ſurrounds the central one? As, by the former ſuppoſition, the luminous central point muſt far exceed the ſtandard of what we call a ſtar, ſo, in the latter, the ſhining matter about the center will be much too ſmall to come under the ſame denomination; we therefore either have a central body which is not a ſtar, or have a ſtar which is involved in a ſhining fluid, of a nature totally unknown to us.

I can adopt no other ſentiment than the latter, ſince the probability is certainly not for the exiſtence of ſo enormous a body as would be required to ſhine like a ſtar of the 8th magnitude, at a diſtance ſufficiently great to cauſe a vaſt ſyſtem of ſtars to put on the appearance of a very diluted, milky nebuloſity.

But what a field of novelty is here opened to our conceptions! A ſhining fluid, of a brightneſs ſufficient to reach us from the remote regions of a ſtar of the 8th, 9th, 10th, 11th, or 12th magnitude, and of an extent ſo conſiderable as to take up 3, 4, 5, or 6 minutes in diameter! Can we compare it to the coruſcations of the electrical fluid in the aurora borealis? Or to the more magnificent cone of the zodiacal light as we ſee it in ſpring or autumn? The latter, notwithſtanding I have obſerved it to reach at leaſt 90 degrees from the ſun, is yet of ſo little extent and brightneſs as probably not to be perceived even by the inhabitants of Saturn or the Georgian planet, and muſt be utterly inviſible at the remoteneſs of the neareſt fixed ſtar.

More extenſive views may be derived from this proof of the exiſtence of a ſhining matter. Perhaps it has been too haſtily ſurmiſed that all milky nebuloſity, of which there is ſo much in the heavens, is owing to ſtarlight only. Theſe nebulous ſtars may ſerve as a clue to unravel other myſterious phænomena. If the ſhining fluid that ſurrounds them is not ſo eſſentially connected with theſe nebulous ſtars but that it can alſo exiſt without them, which ſeems to be ſufficiently probable, and will be examined hereafter, we may with great facility explain that very extenſive, teleſcopic nebuloſity, which, as I mentioned before, is expanded over more than ſixty degrees of the heavens, about the conſtellation of Orion; a luminous matter accounting much better for it than cluſtering ſtars at a diſtance. In this caſe we may alſo pretty nearly gueſs at its ſituation, which muſt commence ſomewhere about the range of the ſtars of the 7th magnitude, or a little farther from us, and extend unequally in ſome places perhaps to the regions of thoſe of the 9th, 10th, 11th, and 12th. The foundation for this ſurmiſe is, that, not unlikely, ſome of the ſtars that happen to be ſituated in a more condenſed part of it, or that perhaps by their own attraction draw together ſome quantity of this fluid greater than what they are intitled to by their ſituation in it, will, of courſe, aſſume the appearance of cloudy ſtars; and many of thoſe I have named are either in this ſtratum of luminous matter, or very near it.

We have ſaid above, that in nebulous ſtars the exiſtence of the ſhining fluid does not ſeem to be ſo eſſentially connected with the central points that it might not alſo exiſt without them. For this opinion we may aſſign ſeveral reaſons. One of them is the great reſemblance between the chevelure of theſe ſtars and the diffuſed extenſive nebuloſity mentioned before, which renders it highly probable that they are of the ſame nature. Now, if this be admitted, the ſeparate exiſtence of the luminous matter, or its independance on a central ſtar, is fully proved. We may alſo judge, very confidently, that the light of this ſhining fluid is no kind of reflection from the ſtar in the center; for, as we have already obſerved, reflected light could never reach us at the great diſtance we are from ſuch objects. Beſides, how impenetrable would be an atmoſphere of a ſufficient denſity to reflect ſo great a quantity of light? And yet we obſerve, that the outward parts of the chevelure are nearly as bright as thoſe that are cloſe to the ſtar; ſo that this ſuppoſed atmoſphere ought to give no obſtruction to the paſſage of the central rays. If, therefore, this matter is ſelf-luminous, it ſeems more fit to produce a ſtar by its condenſation than to depend on the ſtar for its exiſtence.

Many other diffuſed nebuloſities, beſides that about the conſtellation of Orion, have been obſerved or ſuſpected; but ſome of them are probably very diſtant, and run out far into ſpace. For inſtance, about 5 minutes in time preceding ξ Cygni, I ſuſpect as much of it as covers near four ſquare degrees; and much about the ſame quantity 44′ preceding the 125 Tauri. A ſpace of almoſt 8 ſquare degrees, 6′ preceding α Trianguli, ſeems to be tinged with milky nebuloſity. Three minutes preceding the 46 Eridani, ſtrong, milky nebuloſity is expanded over more than two ſquare degrees. 54′ preceding the 13th Canum venaticorum, and again 48′ preceding the ſame ſtar, I found the field of view affected with whitiſh nebuloſity throughout the whole breadth of the ſweep, which was 2° 39′. 4′ following the 57 Cygni, a conſiderable ſpace is filled with faint, milky nebuloſity, which is pretty bright in ſome places, and contains the 37th nebula of my Vth claſs, in the brighteſt part of it. In the neighbourhood of the 44th Piſcium, very faint nebuloſity appears to be diffuſed over more than 9 ſquare degrees of the heavens. Now, all theſe phænomena, as we have already ſeen, will admit of a much eaſier explanation by a luminous fluid than by ſtars at an immenſe diſtance.

The nature of planetary nebulæ, which has hitherto been involved in much darkneſs, may now be explained with ſome degree of ſatisfaction, ſince the uniform and very conſiderable brightneſs of their apparent diſk accords remarkably well with a much condenſed, luminous fluid; whereas to ſuppoſe them to conſiſt of cluſtering ſtars will not ſo completely account for the milkineſs or ſoft tint of their light, to produce which it would be required that the condenſation of the ſtars ſhould be carried to an almoſt inconceivable degree of accumulation. The ſurmiſe of the regeneration of ſtars, by means of planetary nebulæ, expreſſed in a former Paper, will become more probable, as all the luminous matter contained in one of them, when gathered together into a body of the ſize of a ſtar, would have nearly ſuch a quantity of light as we find the planetary nebulæ to give. To prove this experimentally, we may view them with a teleſcope that does not magniſy ſufficiently to ſhew their extent, by which means we ſhall gather all their light together into a point, when they will be found to aſſume the appearance of ſmall ſtars; that is, of ſtars at the diſtance of thoſe which we call of the 8th, 9th, or 10th magnitude. Indeed this idea is greatly ſupported by the diſcovery of a well defined, lucid point, reſembling a ſtar, in the center of one of them: for the argument which has been uſed, in the caſe of nebulous ſtars, to ſhew the probability of the exiſtence of a luminous matter, which reſted upon the diſparity between a bright point and its ſurrounding ſhining fluid, may here be alledged with equal juſtice. If the point be a generating ſtar, the further accumulation of the already much condenſed, luminous matter, may complete it in time.

How far the light that is perpetually emitted from millions of ſuns may be concerned in this ſhining fluid, it might be preſumptuous to attempt to determine; but, notwithſtanding the unconceivable ſubtilty of the particles of light, when the number of the emitting bodies is almoſt infinitely great, and the time of the continual emiſſion indefinitely long, the quantity of emitted particles may well become adequate to the conſtitution of a ſhining fluid, or luminous matter, provided a cauſe can be found that may retain them from flying off, or reunite them. But ſuch a cauſe cannot be difficult to gueſs at, when we know that light is ſo eaſily reflected, refracted, inflected, and deflected; and that, in the immenſe range of its courſe, it muſt paſs through innumerable ſyſtems, where it cannot but frequently meet with many obſtacles to its rectilinear progreſſion. Not to mention the great counteraction of the united attractive force of whole ſidereal ſyſtems, which muſt be continually exerting their power upon the particles while they are endeavouring to fly off. However, we ſhall lay no ſtreſs upon a ſurmiſe of this kind, as the means of verifying it are wanting: nor is it of any immediate conſequence to us to know the origin of the luminous matter. Let it ſuffice, that its exiſtence is rendered evident, by means of nebulous ſtars.

I hope it will be found, that in what has been ſaid I have not launched out into hypothetical reaſonings; and that facts have all along been kept ſufficiently in view. But, in order to give every one a fair opportunity to follow me in the reflections I have been led into, the place of every object from which I have argued has been purpoſely added, that the validity of what I have advanced might be put to the proof by thoſe who are inclined, and furniſhed, with the neceſſary inſtruments to undertake an attentive and repeated inſpection of the ſame phænomena.

W. HERSCHEL.

Slough, Jan. 1, 1791.

V. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland; *by* Thomas Barker, *Eſq.*; *with the Rain in* Hampſhire *and* Surrey; *for the Year* 1789. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read Feb. 17, 1791.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.				Surrey.	Hampſhire.	
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	S. Lambeth.	Sel-bourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30,25	28,00	29,23	47	27	36	47	13½	32	2,604	2,41	4,48	2,98
	Aftern.				50½	28	37	51½	21½	37				
Feb.	Morn.	29,79	28,13	29,18	47½	37½	42	46	31	37	1,847	2,51	4,11	3,31
	Aftern.				47½	39	43	51½	36½	44				
Mar.	Morn.	29,67	28,50	29,25	40½	34½	37½	37	22	32	1,152	2,32	2,47	2,30
	Aftern.				40½	36	38	46½	33	40				
Apr.	Morn.	29,70	28,61	29,28	53½	39½	46	51	32	41	1,010	1,24	1,81	1,58
	Aftern.				56½	41½	48	67	43	53				
May	Morn.	29,80	29,12	29,42	63	48	55½	59½	42½	50	1,677	2,80	4,05	4,03
	Aftern.				63½	49	57	71½	45½	63½				
June	Morn.	29,82	28,92	29,38	64	53½	58	62½	49	53	4,447	3,66	4,24	5,03
	Aftern.				66	55	59	77½	58	67				
July	Morn.	29,63	29,10	29,39	63½	56½	60½	62	49½	57	4,259	2,77	3,69	3,95
	Aftern.				65	58½	61½	78½	59½	69				
Aug.	Morn.	29,90	29,25	29,61	65	55	62	62½	50	57	0,331	1,91	0,99	0,33
	Aftern.				68	59½	63½	74½	60½	69				
Sept.	Morn.	29,88	28,85	29,40	63	52½	57½	57½	42	50½	2,846	1,87	2,82	3,58
	Aftern.				64	53½	59	72	55	63				
Oct.	Morn.	29,84	28,52	29,22	55½	43½	50	50	32	44	4,931	3,54	5,04	3,35
	Aftern.				57	43½	51	62	39	52				
Nov.	Morn.	29,90	28,25	29,26	44	38	42	43	30½	36½	1,199	—	3,67	1,69
	Aftern.				45	38½	42	50½	36½	43				
Dec.	Morn.	30,04	28,35	29,32	48	37½	43	50½	30½	40	1,699	1,51	4,63	3,48
	Aftern.				48½	38½	44	52½	34½	44				
										Inches	28,002		42,00	35,61

MANY have thought, that a hard winter generally comes after a wet ſummer, when the ground has been chilled with cold and rain; ſo the great froſt in 1740 came after a cold wet ſummer 1739; but it was not ſo in 1788, which was in general a dry ſummer, and in ſome places very much ſo, and not defective in hot weather. The whole year's rain was 17 inches, which is leſs than any year ſince 1750. The autumn was dry and fine, and ſo free from froſts, that ſeveral autumnal flowers were flowering in the garden when the froſt began, which are often cut off a month or more earlier.

The laſt day I have mentioned as mild was Nov. 22. and the firſt day of the froſt Nov. 26; it began in Hampſhire three days ſooner. But for more than the firſt fortnight it was very moderate, often freezing at night and thawing in the day; and it began to be ſevere Dec. 12.; was exceeding cold, and ſome very ſharp winds, and, as there was then no conſiderable ſnow, it entered deep into the ground. An imperfect thaw, Dec. 24. and 25. was followed by a great ſnow, and the froſt returned as hard as ever. There came ſnow again ſeveral times; and it lay ſo looſe and hard frozen on the ground, as to be often driving about into very great heaps; till January 13, 1789, when, in one of the worſt days of all for ſtorm, ſnow, and driving, it began to thaw at night; yet the ground was ſo hard frozen and cold, eſpecially where paths had been ſwept, that the moiſture of the air continued freezing for ſome days longer in cakes of ice on the bare ground and ſtones.

It was remarkable that this froſt was ſeverer ſouthward than northward, in France than England. It froze over our great rivers, yet not ſo as to venture to build on them, as was done in 1740. It alſo froze the much larger rivers of Europe, and was in moſt parts a ſevere winter; but to the north and north-weſt of us, it appears not to have been ſo, and that it was moderate in Scotland, and was chiefly great ſnows in Ireland.

The air was very ſharp during the froſt, and it froze exceeding hard, even within doors; yet I have ſeen the thermometer lower in other froſts than it was in this. The loweſt I ſaw was Jan. 12. at 13°½; but in Jan. 1786, it was down at 11°½; in the ſevere froſt Jan. 1776, it was 11° and 10°; Feb. 1784, at 9°; Dec. 1783, at 8°½; and one morning, Feb. 12, 1771, it was down at 4°, which is the loweſt I ever ſaw it.

The froſt from Dec. 1739 to Feb. 1740 was the greateſt I ever knew, and many trees, ſhrubs, and plants, were killed by it, or nearly ſo; but in general they eſcaped this winter. In moſt gardens every plant of roſemary was killed that year, few or none were much hurt this winter. In 1740, the wheat in many countries received great damage; in this froſt I believe it was very little hurt. Moſt of the branches of common furze were killed in 1740, many quite down to the ground, and ſome were entirely deſtroyed; and I have known many ſuffer much, and ſome killed, in 1776, and other hard winters; but this year only a few were much hurt. Many turneps were frozen in the fields this year, a huſbandry not ſo much practiſed in 1740; and many apples and pears were frozen in the houſe, where they were not carefully preſerved; yet, I think, there was leſs of this than was apprehended. Artichokes, I think, ſuffered as much as moſt things in the garden; ſome were killed, and others ſo hurt as to prevent their bearing, but many eſcaped; but there was a greater loſs among them in 1740.

Many walnut-trees were ſplit from the collar to the root by the great froſt in 1740, ſo that a knife might be thruſt in eight or ten inches; the clefts cloſed again in ſummer, but never united. They grew out into a ſeam higher than the reſt of the wood, and have ſo continued ever ſince, yet without hurt either to the growth or bearing of the trees, and ſeveral of them were again ſplit by the late froſt. Such ſeams may be ſeen on many walnut-trees where the cauſe is not remembered. It ſeems odd, that clefts which did not affect the growth of a tree ſhould yet never heal, but remain an indelible mark for ſo long a time; but it ſeems to me, that if wood is once parted, it will never join again, for the whole growth of a tree is between the bark and the wood; but the cleft may be covered over with new wood, as we ſometimes ſee a branch broken off when the tree is young covered over with a great thickneſs of timber. I have known ſeveral aſh trees ſplit by lightning without a twig being killed; but, in ſeveral years they ſtood afterward, there were no ſigns of their uniting again. In an oak, which had ſome bark ſtruck off by the ſame means laſt year, but is not ſplit that I know of, ſome of the leſſer branches withered.

One thing ſeems to have been more common this year than in 1740, and that was the loſs of fiſh in ponds. Where the ponds were deep, well ſupplied with water, and the ice unbroken, no fiſh died; but where the water was ſhallow, little or no current, and the ice kept broken, many periſhed; and in ſome places, where all theſe cauſes concurred, they were all killed. The difference might ariſe from the want of water this year after a dry autumn, of which there was no defect in 1739. Carp were taken out of a pond where the ice was broken, frozen crooked and ſtiff without the leaſt motion, and ice hanging about them; but, being laid on dry ſtraw in a cellar, they all recovered. Some have made a practice of breaking the ice to give the fiſh air; but, by all the examples I heard of in this froſt, it appears to be a great miſtake, and that they are much ſafer without it.

Times of diſtreſs will make creatures look out for unuſual food. A land animal does not ſeem naturally to live on fiſh; but in this froſt a fox was frequently tracked to the mouth of a covered drain, juſt deep enough to let in the fiſh from a pond, and was one morning ſeen eating one on the bank, the blood of which was traced on the ſnow to the drain, ſhewing plainly that it came from thence.

After the froſt broke it was windy and wet, and the air did not become mild for ten days or a fortnight; and the ground was ſo dry within, that the melting of the ſnow and the rain together did not make great floods. It continued often windy and ſhowery till March; but from the latter end of January it was, as to warmth, mild. With March the winter returned, there was almoſt conſtant north winds, froſty mornings or quite froſt, and ſometimes hard and frequent ſnow. This ſtopped the ſeed time, which was begun before, and made it late; but when it did come, it was good. The ſpring was backward, and frequent froſty mornings; but mended gradually, and things came on, though ſlowly, and the ſpring was dry till the middle of May.

Then came a ſhowery and fine growing time for three weeks, and after a ſhort time it became hot and dry. The middle of June a wet ſeaſon began with a very great rain, and it was wet without interruption till the end of July. There was a great deal of hay made this year, but little of it right good. Many of the meadows were flooded; the uplands could not be well gotten for

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the wet; there were great eddiſhes, and a vaſt deal of late graſs, much of which was well made into hay in Auguſt, which was a fine dry ſummer month; but ſuch late graſs is not ſo good as the earlier. This month was rather an interruption than a ceaſing of the wet ſeaſon, which began again the beginning of September, and continued to the end of the year, but the rain fell in leſs quantity from the middle of October to the middle of December; but the ſeaſon has been very open and mild, ſcarcely any froſt, and the ground ſtill green at Chriſtmas.

The ſummer was ſo wet there was very little honey this year. The growth of trees was very great; many ſhoots were three and four feet long or more; and a young aſh tree, of ſix feet high, in the garden, made a ſhoot five feet and an half long, and as thick as a finger. The grain was very rank and foul; there was bulk enough upon the ground, but it yielded very badly to the threſher, perhaps not greatly amiſs to the acre. Harveſt being late, but little was got in during the fine month of Auguſt. the white corn was however carried in tolerable order; but a great many of the beans and peaſe were ſpoiled.

VI. *Obſervations on certain horny Excreſcences of the Human Body.* *By* Everard Home, *Eſq. F. R. S.*

Read February 17, 1791.

THE hiſtory of diſeaſes belongs not properly to the province of the naturaliſt or philoſopher; it is intimately connected with the inquiries of the phyſician and anatomiſt; but when diſeaſe becomes a cauſe of the formation of parts ſimilar to others exiſting in nature, but rendered uncommon by novelty of ſituation, or produced in animals to which they are not naturally appropriated, it may be conſidered as having inſtituted a monſtrous variety, highly deſerving of attention from the naturaliſt.

To deſcribe ſuch varieties is indeed more fully the office of natural hiſtory than of medicine; but the inveſtigation of diſeaſes which are found to ſubvert the ordinary laws of nature reſpecting the ſituation or production of parts in an animal body, undoubtedly belongs to the medical practitioner.

By theſe conſiderations I have been induced to lay before the Royal Society the following account of a diſeaſe which occurs ſometimes in the human body, very remarkable in its effects, but very little underſtood as to its cauſe; namely, the production of an excreſcence ſimilar to a horn. So curious a phænomenon has naturally attracted the attention of the ignorant as well as the philoſopher; and the individuals who have had the misfortune to be ſubject to this diſeaſe have been conſidered as monſters.

Horny excreſcences ariſing from the human head have not only occurred in this country, but have been met with in ſeveral other parts of Europe; and the horns themſelves have been depoſited as valuable curioſities in the firſt collections in Europe.

In giving the hiſtory of a diſeaſe ſo rare in its occurrence, and in its effects ſo remarkable as almoſt to exceed belief, it might be thought right to take ſome pains in bringing proofs to aſcertain that ſuch a diſeaſe does really exiſt: I conſider the doing ſo as leſs neceſſary at preſent, there being two women now alive, and reſiding in England, who are affected by the complaint. I ſhall, however, in the courſe of this Paper, bring other evidence from the teſtimony of the moſt reſpectable authors who have conſidered this ſubject.

The two following caſes contain a very accurate and diſtinct hiſtory of the progreſs of the diſeaſe through its different ſtages, and make any further detail of the ſymptoms intirely unneceſſary.

Mrs. LONSDALE, a woman 56 years old, a native of Horncaſtle in Lincolnſhire, fourteen years ago, obſerved a moveable tumor on the left ſide of her head, about two inches above the upper arch of the left ear, which gradually increaſed in the courſe of four or five years to the ſize of a pullet's egg, when it burſt, and for a week continued to diſcharge a thick, gritty fluid. In the center of the tumor, after the fluid was diſcharged, ſhe perceived a ſmall ſoft ſubſtance, of the ſize of a pea, and of a reddiſh colour on the top, which at that time ſhe took for proud fleſh. It gradually increaſed in length and thickneſs, and continued pliable for about three months, when it firſt began to put on a horny appearance. In two years and three months from its firſt formation, made deſperate by the increaſed violence of the pain, ſhe attempted to tear it from her head; and with much difficulty, and many efforts, at length broke it in the middle, and afterwards tore the root from her head, leaving a conſiderable depreſſion which ſtill remains in the part where it grew. Its length altogether is about five inches, and its circumference at the two ends about one inch; but in the middle rather leſs. It is *curled* like a ram's horn contorted, and in colour much reſembling iſinglaſs.

From the lower edge of the depreſſion another horn is now growing, of the ſame colour with the former, in length about three inches, and nearly the thickneſs of a ſmall gooſe quill; it is leſs contorted, and lies cloſe upon the head.

A third horn, ſituated about the upper part of the lambdoidal ſuture, is much curved, above an inch in length, and more in circumference at its root: its direction is backwards, with ſome elevation from the head. At this place two or three ſucceſſive horns have been produced, which ſhe has conſtantly torn away; but, as freſh ones have ſpeedily followed, ſhe leaves the preſent one unmoleſted in hopes of its dropping off.

Beſides theſe horny excreſcences, there are two tumors, each the ſize of a large cockle; one upon the upper part, the other about the middle of the left ſide of the head; both of them admit of conſiderable motion, and ſeem to contain fluids of unequal conſiſtence; the upper one affording an obſcure fluctuation, the other a very evident one.

The four horns were all preceded by the ſame kind of incyſted tumours, and the fluid in all of them was gritty; the openings from which the matter iſſued were very ſmall, the cyſts collapſed and dried up, leaving the ſubſtance from which the horn proceeded diſtinguiſhable at the bottom. Theſe cyſts gave little pain till the horns began to ſhoot, and then became very diſtreſſing, and continued with ſhort intervals till they were removed. This caſe is drawn up by the ſurgeon who attended the woman for many years, which gave him frequent opportunities of ſeeing the diſeaſe in its different ſtages, and acquiring an accurate hiſtory of its ſymptoms.

Mrs. ALLEN, a middle-aged woman, reſident in Leiceſterſhire, had an incyſted tumor upon her head, immediately under the ſcalp, very moveable, and evidently containing a fluid. It gave no pain unleſs preſſed upon, and grew to the ſize of a ſmall hen's egg. A few years ago it burſt, and diſcharged a fluid; this diminiſhed in quantity, and in a ſhort time a horny excreſcence, ſimilar to thoſe above mentioned, grew out from the orifice, which has continued to increaſe in ſize; and in the month of November 1790, the time I ſaw it, was about five inches long, and a little more than an inch in circumference at its baſe. It was a good deal contorted, and the ſurface very irregular, having a laminated appearance. It moved readily with the ſcalp, and ſeemed to give no pain upon motion; but, when much handled, the ſurrounding ſkin became inflamed. This woman came to London, and exhibited herſelf as a ſhow for money; and it is highly probable, that ſo rare an occurrence would have ſufficiently excited the public attention to have made it anſwer her expectations in point of emolument, had not the circumſtance been made known to her neighbours in the country, who were much diſſatisfied with the meaſure, and by their importunity obliged her huſband to take her into the country.

That the caſes which I have related may not be conſidered as peculiar inſtances from which no concluſions can be drawn, it may not be amiſs to take notice of ſome of the moſt remarkable hiſtories of this kind, mentioned by authors, and ſee how far they agree with thoſe I have ſtated, in the general characters that are ſufficiently obvious to ſtrike a common obſerver; for the vague and indefinite terms in which authors expreſs themſelves on this ſubject ſhew plainly, that they did not underſtand the nature of the diſeaſe, and their accounts of it are not very ſatisfactory to their readers.

In the *Ephemerides Academiæ Naturæ Curioſorum* there are two caſes of horns growing from the human body. One of theſe inſtances was a German woman [*], who had ſeveral ſwellings, or ganglions, upon different parts of her head, from one of which a horn grew. The other was a nobleman [†], who had a ſmall tumor, about the ſize of a nut, growing upon the parts covering the two laſt or lowermoſt vertebræ of the back. It continued for ten years, without undergoing any apparent change; but afterwards enlarged in ſize, and a horny excreſcence grew out from it.

In the Hiſtory of the Royal Society of Medicine [‡], there is an account of a woman, 97 years old, who had ſeveral tumors on her head, which had been 14 years in growing to the ſtate they were in at that time: ſhe had alſo a horn which had originated from a ſimilar tumor. The horn was very moveable, being attached to the ſcalp, without any adheſion to the ſcull. It was ſawn off, but grew again, and although the operation was repeated ſeveral times, the horn always returned.

Bartholine, in his Epiſtles [§], takes notice of a woman who had a tumor under the ſcalp, covering the temporal muſcle. This gradually enlarged, and a horn grew from it, which had become twelve inches long in the year 1646, the time he ſaw it. He gives us a repreſentation of it, which bears a very accurate reſemblance to that which I have mentioned to have ſeen in November 1790. No tumor or ſwelling is expreſſed in the figure; but the horn is coming directly out from the ſurface of the ſkin.

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[*] Ephem. Acad. Nat. Cur. Dec. iii. An. V. Append. p. 148.

[†] Ibid. Dec. i. An. I. Obſervat. 30.

[‡] Hiſtoire de la Société Royale de Médecine, 1776, p. 316.

[§] Epiſtol. Thom. Barthol.

In the Natural Hiſtory of Cheſhire *, a woman is mentioned to have lived in the year 1668, who had a tumor or wen upon her head for 32 years, which afterwards enlarged, and two horns grew out of it; ſhe was then 72 years old.

There is a horny excreſcence in the Britiſh Muſeum, which is eleven inches long, and two inches and a half in circumference at the baſe, or thickeſt part. The following account of this horn I have been favoured with by Dr. GRAY, taken from the records of the Muſeum. A woman, named FRENCH, who lived near Tenterden, had a tumor or wen upon her head, which increaſed to the ſize of a walnut; and in the 48th year of her age this horn began to grow, and in four years arrived at its preſent ſize †.

There are many ſimilar hiſtories of theſe horny excreſcences in the authors I have quoted, and in ſeveral others; but thoſe mentioned above are the moſt accurate and particular with reſpect to their growth, and in all of them we find the origin was from a tumor, as in the two caſes I have related; and although the nature of the tumor is not particularly mentioned, there can be no doubt of its being of the incyſted kind, ſince in its

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* LEE's Natural Hiſtory of Lancaſhire and Cheſhire.

† The following extract is taken from the Minutes of the Royal Society, Feb. 14, 1704-5.

" A Letter was read from Dr. CHARIERE, at Barnſtaple, concerning a horn, " ſeven inches long, cut off the ſecond vertebra of the neck of a woman in that " neighbourhood.

" Dr. GREGORY ſaid, that one of ſeven inches long, and of a dark brown " colour, was cut off from a woman's temple at Edinburgh.

" Dr. NORRIS ſaid, that two horns had been cut off from a woman's head " in Cheſhire.

progreſs it exactly reſembled them, remaining ſtationary for a long time, and then coming forwards to the ſkin; and the horn being much ſmaller than the tumor previouſly to the formation of the horn, is a proof that the tumor muſt have burſt, and diſcharged its contents.

From the foregoing account it muſt appear evident, that theſe horny excreſcences are not to be ranked among the appearances called *luſus naturæ*: nor are they altogether the product of diſeaſe, although undoubtedly the conſequence of a local diſeaſe having previouſly exiſted; they are, more properly ſpeaking, the reſult of certain operations in the part for its own reſtoration; but the actions of the animal œconomy being unable to bring them back to their original ſtate, this ſpecies of excreſcence is formed as a ſubſtitute for the natural cuticular covering.

To explain the manner in which theſe horns are formed, it will be neceſſary to conſider the nature of incyſted tumors a little more fully; and in doing ſo we ſhall find, that this particular ſpecies does not differ in its principle, nor materially in its effects, from many others which are not uncommonly met with in the human body, as well as in thoſe of many other animals, which, as they are more frequent in their occurrence, are alſo much better underſtood.

Incyſted tumors differ exceedingly among themſelves, both in the nature of their contents, and in their progreſs towards the external ſurface of the body. Many of them have no reference to our preſent purpoſe; it is only the more indolent kind to which I mean now to advert: ſome of theſe, when examined, are not found to contain a fluid, but a ſmall quantity of thick, curd-like matter, mixed with cuticle broken down into ſmall parts, and upon expoſing the internal ſurface of the cyſt, it is found to have an uniform cuticular covering adhering to it, ſimilar to that of the cutis on the ſurface of the body, from which it only differs in being thinner, and more delicate, bearing a greater reſemblance to that which covers the lips. Others of this kind, inſtead of having cuticle for their contents, are filled with hair mixed with a curdled ſubſtance, or hair without any admixture whatever, and have a ſimilar kind of hair growing upon their internal ſurface, which is likewiſe covered with a cuticle. Theſe cuticular incyſted tumors were, I believe, firſt accurately examined by Mr. HUNTER, to whom we are likewiſe indebted for an explanation of the mode in which the parts acquire this particular ſtructure.

Mr. HUNTER conſiders the internal ſurface of the cyſt to be ſo circumſtanced reſpecting the body, as to loſe the ſtimulus of being an internal part, and receive the ſame impreſſion from its contents, either from their nature, or the length of application, as the ſurface of the ſkin does from its external ſituation. It therefore takes on actions ſuited to ſuch ſtimuli, undergoes a change in its ſtructure, and acquires a diſpoſition ſimilar to the cutis, and is conſequently poſſeſſed of the power of producing cuticle and hair. What the mode of action is, by which this change is brought about, is not eaſily determined; but from the indolence of theſe complaints, it moſt probably requires a conſiderable length of time to produce it. That the lining of the cyſt really does poſſeſs powers ſimilar to cutis, is proved by the following circumſtances: that it has a power of forming a ſucceſſion of cuticles like the common ſkin; and what is thrown off in this way is found in the cavity of the cyſt. It has a ſimilar power reſpecting hair, and ſometimes the cavity is filled with it, ſo great a quantity has been ſhed by the internal ſurface. Beſides theſe circumſtances, the hair found in the cyſt correſponds in appearance with that which grows upon the body of the animal; and when incyſted tumors of this kind form in ſheep, they contain wool. What is ſtill more curious, when ſuch cyſts are laid open, the internal ſurface undergoes no change from expoſure, the cut edges cicatrize, and the bottom of the bag remains ever after an external ſurface. Different ſpecimens, illuſtrative of the above-mentioned circumſtances, are preſerved in Mr. HUNTER's collection of diſeaſes.

The cyſts that produce horny excreſcences (which are only another modification of cuticle) are very improperly conſidered as giving riſe to horns; for if we examine the mode in which this ſubſtance grows, we ſhall find it the ſame with the human nails, coming directly out from the ſurface of the cutis. It differs from the nails in not being ſet upon the ſkin by a thin edge, but by a ſurface of ſome breadth, with a hollow in the middle, exactly in the ſame manner as the horn of the rhinoceros *; at leaſt this is evidently the caſe in the ſpecimen preſerved in the Britiſh Muſeum, and in one which grew out from the tip of a ſheep's ear; they are alſo ſolid, or nearly ſo, in their ſubſtance.

This mode of growth is very different from that of horns, which are all formed upon a core, either of bone or ſoft parts, by which means they have a cavity in them; a ſtructure peculiar to this kind of cuticular ſubſtance.

Incyſted tumors in different animals would appear, from theſe obſervations, to be confined in their production to the

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* The horn of the Rhinoceros is a cuticular appendage to the ſkin, ſimilar to nails and other cuticular excreſcences, being in no reſpect allied to horns but in the external appearance.

cuticular ſubſtance proper to the animal in which they take place; for, although cuticle, hair, nail, hoof, and horn, are equally productions of animal ſubſtance, only differing in trivial circumſtances from each other, we do not find in the human ſubject any inſtance of an incyſted tumor containing a ſubſtance different from the cuticle, hair, and nails of the human body, to which laſt the horny excreſcences, the ſubject of the preſent Paper, are certainly very cloſely allied, both in growth, ſtructure, and external appearance; and when of ſome length, they are found to be ſo brittle as to break in two, upon being roughly handled, which could not happen either to hoof or horn. In the ſheep they produce wool inſtead of hair; and in one inſtance in that animal, where they gave riſe to an horny excreſcence, it was leſs compact in its texture, and leſs brittle than ſimilar appearances in the human ſubject; upon being divided longitudinally, the cut ſurface had more the appearance of hoof, and was more varied in its colour than nail.

Incyſted tumors being capable of producing horns, upon the principle we have laid down, is contrary to the uſual operations of nature; for horns are not a production from the cutis, and although not always formed upon a bony core, but frequently upon a ſoft pulp, that ſubſtance differs from common cutis in its appearance, and extends a conſidera- way into the horn: it is probable, that this pulp requires a particular proceſs for its formation *.

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* A ſheep, about four years old, had a large horn, three feet long, growing upon its flank. It had no connection with bone, and appeared to be only attached to the external ſkin. It dropped off in conſequence of its weight having produced ulceration in the ſoft parts to which it adhered. Upon examining it there was a fleſhy ſubſtance, ſeven inches long, of a fibrous texture filling up its cavity upon which the horn had been formed.

I ſhall conclude this Paper by obſerving, that the caſes of horns, as they are commonly termed, upon the human head, are no more than cuticular productions ariſing from a cyſt, which in its nature is a variety of thoſe tumors deſcribed by Mr. HUNTER under the general name of cuticular incyſted tumors *.

Theſe incyſted tumors, when conſidered as varieties of the ſame diſeaſe, form a very complete and beautiful ſeries of the different modes by which the powers of the animal œconomy produce a ſubſtitute for the common cuticle upon parts which have been ſo much affected by diſeaſe as to be unable to reſtore themſelves to a natural ſtate.

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* The principle upon which the production of theſe excreſcences depends being once explained, the modes of preventing their formation, and removing them when formed, will be readily underſtood, the deſtruction of the cyſt being all that is required for that purpoſe. This may be done before the tumor opens externally, or even after the excreſcence has begun to ſhoot out, and will be better effected by diſſection than eſcharotics, ſince the ſucceſs of the operation depends upon the whole of the bag being removed.

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VII. *Conſiderations on the Convenience of meaſuring an Arch of the Meridian, and of the Parallel of Longitude, having the Obſervatory of* Geneva *for their common Interſection. By* Mark Auguſtus Pictet, *Profeſſor of Philoſophy in the Academy of* Geneva; *in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 24, 1791.

SIR,

THE accurate knowledge of the dimenſions and true figure of the earth is not a matter of mere curioſity. Aſtronomy and navigation are ſo cloſely connected with it, that the philoſophers of the preſent century have purſued this enquiry through the moſt diſcouraging difficulties; and governments themſelves have contributed conſiderable ſums towards its ſucceſs.

We know that, notwithſtanding theſe efforts, the end is not yet obtained. There are five different concluſions on this ſubject; one of which is given by SIR ISAAC NEWTON's theory; the others are the reſult of four different meaſurements, which appear the moſt creditable among thoſe that have been performed. The extremes give $\frac{1}{132}$ and $\frac{1}{304}$ for the difference between the polar and equatorial diameters of the earth, that is, two fractions, one of which is more than double the other. The cauſe of theſe diſagreements is yet unknown; perhaps the figure of the earth is really irregular; perhaps the ſeveral meaſurements have not been executed with the very minute exactneſs requiſite in ſo nice and ſo important an undertaking.

The liberal and well-conducted operations carried on by the Royal Society, under the direction of the late General ROY, for the trigonometrical determination of the diſtance between the Obſervatories of Greenwich and Paris, render this laſt ſuppoſition extremely probable. It now ſeems evident, that the ſubſtances employed before for the actual meaſurement of the baſes muſt have been influenced in their length by pyrometrical and hygrometrical effects, which were either unknown or ill-eſtimated at that time. The inſtruments alſo for obſerving the celeſtial and terreſtrial angles were far from the perfection to which they have ſince been brought. In a word, the whole of the work ſhould be again undertaken with the far greater degree of accuracy which is now within our reach.

Struck with the importance of theſe facts, I take the liberty of tranſmitting to you, Sir, for the conſideration of the Royal Society, the preſent plan for meaſuring, by a commiſſion of its Members, an arch of the meridian, and of a parallel of longitude, having the Obſervatory of Geneva for their common point of interſection. Frequent excurſions in our neighbouring mountains have convinced me, not only that the meaſurement could be made, but that it would be perhaps the moſt eaſily executed of any hitherto attempted. The rough ſketch, with its explanation, joined to the preſent Paper, will give ſome account of the particulars which I have been able to collect relative thereto. At preſent I ſhall only lay before the Society a general ſurvey of the undertaking.

The beſt maps place the town of *St. Jean de Maurienne* nearly ſouth of Geneva, at the diſtance of about 58 minutes of latitude. It would be impoſſible to extend the meaſurement farther ſouthwards, the central and inacceſſible chain of the Alps being in the way; but if a greater arch ſhould be deſired, it might be eaſily protracted about 26 minutes north of Geneva.

I lately made an excurſion to St. Jean de Maurienne, to examine whether the poſition of the town would be convenient in other reſpects; and if, in particular, the lateral attraction of ſome neighbouring mountain was not to be feared. It appeared to me, that the place being ſurrounded by mountains of nearly equal maſſes, and ſituated at almoſt equal diſtances, their effects would be hardly perceptible; and, ſuppoſing there ſhould remain any doubt about their influence, this influence might be eaſily aſcertained by zenith diſtances, obſerved at the two extremities of a little plain in which the town is built, and compared with the real diſtance of the ſtations, determined by an actual meaſurement. That town being the reſidence of a biſhop, and containing near 3000 inhabitants, might furniſh the obſervers with a convenient building for the zenith ſector, and the occaſional help and neceſſaries which might be required. The great poſt-road from hence into Italy, over Mount Cenis, paſſing through it, is alſo an advantageous circumſtance.

The diſpoſition and bearing of the vallies from that town, which would be the ſouthernmoſt extremity of the arch, is advantageous for the ſeries of triangles: for I have ſeen from the top of a mountain near St. Jean, called *Le Mont Sapey*, two parallel chains extending to the north on both ſides of the river *Arc*, and there appeared to be, in their ſummits a great choice for convenient ſtations, as far as the confluence of the *Arc* and the river *Iſére* near *Aiguebelle*, from whence the mountains in the parallel of *Chamberi* are all viſible. From this laſt parallel to Geneva, and farther, there are not only no difficulties, but the ſtations are for the greateſt part already determined.

mined. The mountains and lines drawn in black, on the map ſubjoined (Tab.II.), ſhew the ſtations and triangles already obſerved; thoſe in red * are only preſumed, but their places will probably differ little in reality from thoſe which are there indicated. The map, as to the ſituation of mountains, has no pretenſions but to ſuch an exactneſs as may reſult from angles obſerved with a Hadley's ſextant, without a reduction to the horizon; but the ſituation and figure of our lake is exactly true, being the reſult of a minute and accurate ſurvey taken by the late Profeſſor Mallet and myſelf.

The viſible part of the meridian of Geneva is ſoon terminated northwards by the firſt chain of Mount Jura; but the country opens to the N.N.E. and the northern ſtation might be eaſily choſen in ſome place of the *Pays de Vaud*, viſible from the Obſervatory of Geneva, and which could be determined by only one additional triangle. I take the liberty to point out two ſuch places. The one, called *Vincy*, about 16 minutes north of Geneva, and where the circumſtance of my poſſeſſing a country houſe would facilitate the diſpoſitions neceſſary for the aſtronomical obſervations. The other place is the top of a mountain, called the *Dent de Vaulion*, making part of the chain of Mount Jura, and where an occaſional obſervatory might be erected without much difficulty: it is 10 minutes north of Vincy, or 26′ of Geneva. The whole arch from *St. Jean de Maurienne* to this laſt place would be about 1 degree 24 minutes.

The celeſtial obſervations might perhaps be made in the four places above mentioned; and the meridian arch would be thus obtained in three portions, whoſe compariſon with the terreſtrial ſections, meaſured geometrically, would be a proof of the accuracy of the operation.

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* In the engraving of this map, dotted lines are employed, to expreſs whatever was drawn with red in the original.

It is obſervable, on the inſpection of the map, that the chain of triangles lies moſtly on the weſt ſide of the meridian line. I was forced to diſpoſe it thus, to be within reach of the only place where a convenient baſis could be found, *viz.* the marſhes of *Choutagne*. I have ſeen this place, and am well aſſured, that the meaſurement in a convenient ſeaſon would be attended with no difficulty. The diſpoſition and proportion of the baſis of about 29,000 feet traced on the map, are the reſult of operations I have made on the ſpot.

The ſouthern part of the meridian line, viſible from the Obſervatory of Geneva, paſſes over the ſummit of a mountain called *Mount Salève*, where we have a meridian mark, at the diſtance of about 5600 toiſes, and at the height of about 500 toiſes above the level of the lake. I have obſerved from that ſummit, that the ſame line protracted ſouthwards is not intercepted by the mountains but at a great diſtance, and in a place which, as far as I could judge, muſt be near the ſouthern end of the arch. I am ignorant if that place is acceſſible; but if it ſhould happen to be ſo, it would be a fortunate circumſtance, inaſmuch as it would offer a very ſimple, quick, and accurate verification of the direction of the meridian line reſulting from the chain of triangles, by actually protracting the viſual line given immediately by the tranſit inſtrument of the Obſervatory down to the end of the arch, by the help of two intermediate ſtations only. I do not believe that ſuch a verification has ever yet been practiſed.

We ſee hitherto no local difficulties in the meaſurement of an arch of about 84 minutes of the meridian of Geneva. The meaſurement of the parallel of longitude, eaſtwards of the ſame place, ſeems to be of a ſtill eaſier execution, inſomuch that I believe there are few places on earth better diſpoſed for the operation.

The Republic of *Vallais* in Switzerland offers an extenſive, broad, and nearly ſtraight valley, bordered on both ſides by high mountains. It is ſituated about the parallel of Geneva, runs eaſtward for many leagues from the town of *Martigny*, and to the weſtward is ſeparated from the mountains of *Chablais* in Savoy, by a very lofty chain, in which there is an acceſſible ſummit, called *The Glacier de Büet*, or *La Mortine*.

This mountain is placed, as by a miracle, in ſuch a poſition as to be viſible from the Obſervatory of Geneva, and about 10 minutes weſt of it, as alſo from almoſt every elevated ſituation in the *Haut Vallais*. Its ſummit is acceſſible by a much eaſier aſcent than that which was diſcovered by Meſſ. DE LUC; and a ſignal made there by the Indian lights would be viſible eaſt and weſt along the parallel, to the whole diſtance of perhaps two degrees between the two extreme ſtations; for as the obſervations relative to the regulating of the clocks do not require any conſiderable apparatus, they could be performed in the moſt diſtant hamlets from which the ſignal ſhould be viſible.

As to the trigonometrical meaſurement along the parallel, one may conclude, from the triangles delineated on the map, that it might be executed with a ſmaller number of operations than that of the meridian arch. I was aſſiſted in the deſignation of the probable ſtations in the *Vallais* by my ingenious friend Mr. WILD, Director of the Salt Works and Mines of the Republic of Bern, and reſiding at *Bex*; from whence, by frequent excurſions, he has acquired a ſufficient knowledge of the names and poſitions of the neighbouring mountains to enable me to complete the ſketch in the eaſtern part of the parallel.

Should the method, propoſed by the late General ROY*, for aſcertaining the length of the parallel independently of aſtronomical obſervations, be adopted, it might be carried into execution with no great difficulty from the ſummit of the ſame mountain, where we juſt now ſuppoſed the ſignal by the Indian lights to be placed.

The triangles relative to the meaſurement of the parallel make but one ſuite with thoſe of the meridian, and there are four very convenient places along the ſame parallel for meaſuring baſes of verification. They are perfectly level plains, forming the bottom of the valley through which the Rhone flows between the towns of *Aigle* and *Villeneuve*, and between *Martigny* and *Sion*. Their relative ſituations are roughly indicated in the map. The above general conſiderations, together with the particulars which are ſubjoined to the ſketch, ſeem to aſcertain the full practicability of the enterprize. May I be allowed to add a few reflections on its conveniency?

The re-union of the two meaſurements (of latitude and longitude) in the ſame ſpot, is an advantageous circumſtance; and the more ſo, if we conſider that this ſpot lies between the 45 and 46th degree, that is, in the mean latitude between the pole and equator, near which latitude the mean radius of the earth takes place in the well-founded ſuppoſition of its being a ſpheroid. This radius, found by the moſt accurate meaſurement hitherto attempted, would become a ſtandard, and to which the reſults of the equatorial and northern meaſurements being compared, the true figure of the earth would be the better aſcertained.

The poſition or relative longitude of the meridian of Geneva is well determined by a great number of obſerved immerſions and emerſions of the ſatellites of Jupiter, and by ſome occultations of ſtars by the moon. Theſe obſervations were performed by the late Profeſſor J. A. Mallet, Mr. J. Trembley, and myſelf. The greater part of them are already calculated, and their mean reſult muſt be near the truth. The latitude of the ſame place would be aſcertained by the celeſtial obſervations eſſential to the meaſurement, and would verify our determination taken from the obſerved culmination of zenith ſtars. Laſtly, the height of Geneva above the level of the Mediterranean Sea (which is ſuppoſed to be about 196 toiſes) can be obtained with a ſufficient preciſion from the barometrical obſervations which for ſeveral years paſt have been regularly made here.

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* Phil. Tranſ. 1787, p. 216. et ſeq.

The Obſervatory of Geneva, in which, ſince the death of my late friend Profeſſor Mallet, I have continued a ſeries of obſervations, would be at the free diſpoſition of the Commiſſioners whom the Royal Society ſhould think proper to entruſt with the whole work. It is furniſhed with the following inſtruments, and conſequently there would be no occaſion for ſending thoſe of the ſame ſort from England.

1°, A very good tranſit inſtrument of 5 feet, made by Sisson. The teleſcope is achromatic, magnifying about 70 times.

2°, A quadrant of 2½ feet radius with two achromatic teleſcopes and a micrometer ſcrew. The quadrant is moveable on an azimuthal circle. The whole made by the ſame artiſt.

3°, Two aſtronomical clocks, with gridiron pendulums. One remarkably good, made by the late Shelton; the other by Le Paute. This laſt might be carried to any of the ſtations.

4°, An achromatic teleſcope of 10 feet, made by Dollond, with three magnifying powers.

5°, A $3\frac{1}{2}$ feet achromatic, with great aperture, and four magnifying powers, made alſo by DOLLOND, mounted on a parallactic ſtand, and carrying occaſionally an ocular micrometer*.

6°, A very good RAMSDEN's 30-inches achromatic refractor with four powers; beſides ſome other teleſcopes and inſtruments of leſs importance.

The capital inſtruments which, I ſuppoſe, it would be neceſſary to ſend from England, would be one or two zenith ſectors, one or two clocks, and the inſtrument for obſerving the terreſtrial angles, the accuracy of which is of the utmoſt importance. I ſaw in London, three years ago, that made by Mr. RAMSDEN, and employed ſo ſucceſsfully in the late meaſurements from Hounſlow Heath to the French Coaſt. It is a matter of doubt to me, whether this precious inſtrument would be allowed to go out of the kingdom; and even ſhould that be permitted, as far as I can judge from the impreſſion it has left on my memory, it would be too heavy and of too large a bulk to be conveniently carried to the top of mountains. The inſtrument ought perhaps to be made of ſeparable parts eaſily put together, and capable of being ſteadily adjuſted: they might be packed ſeparately in a box, not exceeding the common charge of a man. The boxes could be ſo contrived as to fill a larger one, ſuſpended on ſprings in a ſmall narrow carriage made on purpoſe, and in which all the neceſſary things beſides could be collected. Such a diſpoſition would be very convenient, not only for ſending the inſtruments ſafe from England, but alſo to carry them from one ſtation to another through croſs country roads, with more quickneſs and leſs trouble and danger. A convenient tent, to ſhelter the inſtruments in the ſtations, ſhould be added, or might be made at Geneva.

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* We are indebted for the poſſeſſion of that valuable inſtrument to the munificence of my learned friend M. DE SAUSSURE, who gave it to M. TREMBLEY, to whom it now belongs.

The glaſs rods for the meaſurement of the baſis could be drawn of any convenient ſhape, in a glaſs-houſe not far diſtant from Geneva. The thermometers, and any additions or repairs the apparatus might require, would be duly performed by Meſſ. PAUL, eminent artiſts of this town; and we abound with artiſts of inferior merit.

Thus far, Sir, I foreſee no capital obſtacles to the execution of the plan I have the honour of laying before the Royal Society. All the *data* ſeem in favour of it; and I may add, as another favourable circumſtance, that the ſtar *Capella* of the firſt magnitude culminates between the zeniths of *Geneva* and *St. Jean Maurienne*, and conſequently might be obſerved ſimultaneouſly from both ſtations at any hour in which the ſeaſon of the year would bring its meridian tranſit.

I ſhall conclude this Paper with a few more reflections on the undertaking in general.

Suppoſing it to be the intention of the Royal Society to attempt the inveſtigation of the true figure of the earth, by meaſurements executed in different parts of the globe; this firſt operation, performed in a foreign country, might be looked upon as a kind of previous trial for the obſervers, a ſort of initiation, in which ſome difficulties, owing to the nature of the ſoil, but no true hardſhips, would be encountered. The whole of the work, except perhaps one ſingle ſtation, would be carried on in the King of Sardinia's dominions, and in ſome of the Republics of Switzerland; countries, where no moral nor political obſtacles would be ſtarted, and where the inhabitants, far from being troubleſome, would afford every aſſiſtance and comfort in their power. I may add, that none of the mountains on which I have obſerved, and which are pointed out in the map as likely ſtations, preſent any danger.

If a ſurvey of that kind, executed in a mountainous country, is liable to ſome difficulties, it offers, on the other hand, advantages which perhaps more than overbalance thoſe difficulties.

Firſt, the viſual rays being leſs interrupted, the triangles become larger, and the ſtations fewer in number; whence the labour of the obſervers, and the chances of error, are by ſo much diminiſhed.

Secondly, Theſe ſame viſual rays proceeding through ſtrata of air leſs denſe and more free from the vapours which commonly thicken the lower parts of the atmoſphere, the danger of irregular refractions is by ſo much leſs, and the ſignals may be more diſtinctly perceived from great diſtances.

Beſides thoſe advantages which chiefly concern the meaſurement itſelf, the country would offer facilities for other natural enquiries, not unworthy the attention of philoſophical men, and which might eaſily be united with the capital object of theſe labours, with which object ſome of theſe enquiries are intimately united. I range among them,

The accurate determination of the length of the ſimple pendulum, which beats ſeconds in this mean latitude.

Experiments to be made on the oſcillations at different heights, with an invariable pendulum.

Experiments on the lateral attraction of mountains repeated and varied.

Obſervations on meteors, and ſeveral atmoſpherical phænomena relative to refractions, to heat, to hygrometry, to electricity, &c.

But, above all, the improvement of barometrical meaſurements would moſtly deſerve the attention of the Commiſſioners. Nothing, as it ſeems, is now wanting in the theory of the operation; and it is only from a number of actual obſervations, made at different heights, and with every due precaution, compared with geometrical meaſurement, that the co efficient (either conſtant or variable) to be applied as a correction for the atmoſpherical heat, will be obtained. That reſearch muſt be merely empirical; the effect ſought for being the reſult of many complicated cauſes, ſome of which are yet unknown. The real height of every ſtation being well determined, the time to be ſpent there for other purpoſes would allow a number of barometrical obſervations in varied circumſtances; and from theſe obſervations, rightly compared between themſelves, intereſting and uſeful reſults may juſtly be expected.

I have the honour to be, &c.

MARK AUGUSTE PICTET.

Geneva, Oct. 30, 1790.

Eclairciſſemens ſur la Carte, qui accompagne le Projet de Meſure préſenté à la Societé Royale, par M. A. Pictet.

(See Tab. II.)

DANS l'eſquiſſe de la ſuite de triangles à laquelle le préſent développement ſe rapporte, les montagnes ou les lignes tracées en rouge repréſentent des poſitions tirées des cartes, ou d'une inſpection générale de la direction des montagnes et des vallées *. Ce qui eſt deſſiné en noir indique les poſitions déterminées par des angles et des meſures réellement priſes; mais ſans prétendre cependant à plus de préciſion que la nature de la choſe ne l'exigeoit. Enfin ce qui eſt tracé legèrement en couleur noire montre ſeulement qu'il exiſte des montagnes là où elles ſont ainſi indiquées.

Les deux lignes rouges qui ſe coupent à angles droits dans la carte repréſentent le méridien et le parallèle de l'Obſervatoire de Genève dans l'étendue de la meſure propoſée; c'eſt-à-dire environ 1° 24′ pour le méridien, et environ 2°, pour le parallèle de longitude.

Le ſeul endroit dans lequel j'aye pû trouver un emplacement convenable pour une baſe rélative à la meſure de l'arc du méridien, eſt ſitué à l'oueſt de cet arc, et à-peu-près vis-à-vis du milieu de ſa longueur; ce ſont les marais de Choutagne. Ils offrent une plaine qui s'étend du nord au ſud dans une eſpace d'environ 2 lieues; elle eſt bornée à l'oueſt par le Rhône, et à l'eſt par une montagne très voiſine nommée la montagne de

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* See Note, page 109.

Choutagne, qui ſe prolonge au nord et au ſud beaucoup plus loin que la plaine elle même. Le ſol de cette plaine eſt ſi parfaitement horizontal qu'il paroit n'offrir d'autre élévation que celle qui réſulte de la courbure de la terre et d'une route qu'on a pratiquée au travers de cette même plaine de l'eſt à l'oueſt.

La ligne *a b* repréſente une baſe d'environ 25000 pieds Anglois de longueur, tracée dans cette plaine, et la ligne *b β* une autre baſe priſe dans la même plaine, et dont je parlerai tout à l'heure. Le ſol du marais eſt ferme le long de ces deux lignes, et en s'aidant de quelques ponts dans les endroits où ils ſeroient néceſſaires, la meſure ne ſouffriroit aucune difficulté. Une grande partie de la plaine eſt en prairie pendant l'été; il n'y a que très peu d'eau dans le reſte; je l'ai trouvé encore très praticable au milieu d'Octobre dernier, tems où j'y ai fait les opérations rélatives à la préſente eſquiſſe.

Les deux extrémités *a* et *b* de la baſe ſont marquées par deux bâtimens qui ſeroient utiles dans le voiſinage des deux ſtations: l'un eſt une petite ferme au point *a*, nommée *Chez Thevenet*, et l'autre une Briqueterie à l'extrémité *b*. Ces deux points ſont viſibles l'un de l'autre, et je remarque en paſſant que les inſtrumens pourroient arriver par eau depuis l'Angleterre juſques à la ſtation *b*, par la voye de Marſeille et du Rhône.

J'ajouterai que tout le côté oriental de la baſe, le long du pied de la montagne, eſt très peuplé; c'eſt un vignoble conſidérable, et l'on auroit tout-à-fait à-portée le logement et tous les ſecours néceſſaires au travail de la meſure; et quoique la plaine ſoit marécageuſe, le vignoble que la borde paſſe pour très ſain.

Il n'étoit pas facile de trouver ſur le ſommet de la montagne Choutagne deux ſtations qui réuniſſent les conditions néceſſaires; ſavoir 1mo, D'être viſibles des deux extrémités de la baſe qui ſont abaiſſées de 8 à 10 degrés ſous l'horiſon. 2do, D'être viſibles l'une de l'autre. 3tio, D'être placées de manière qu'on put découvrir les ſommités à l'eſt du côte oppoſé à la baſe. 4to enfin, D'être aſſez diſtantes entre elles pour fournir un côté convenable au premier triangle de la chaîne. Les points marqués *c* et *d* ſont les ſeuls, à ce que je crois, qui réuniſſent ces conditions. J'ai même quelque doute que la montagne de la Caille ſoit viſible du point *c* ſans élever l'inſtrument de quelques pieds. On voit que la diſtance *c d* de ces deux points eſt un peu moindre que la longueur de la baſe, et l'on trouvera ſans doute que l'angle que cette ligne ſouſtend ſur la montagne de la Caille eſt trop petit; il n'eſt effectivement que de 17 degrés, et ſous cette obliquité et à cette diſtance une erreur de 5″ en *c* ou en *d* allongeroit le côté oppoſé de   pieds. Cette conſidération pourroit être jugée ſuffiſante pour engager les commiſſaires à préférer à la ſtation *c* une ſtation *e* que j'indique ſur le ſommet ſeptentrional du mont Colombier, quoique ce point ſoit ſitué ſur le territoire François. La baſe eſt placée très convenablement pour déterminer ce point, et ſi l'on abandonnoit ainſi la ſtation *c*, cette même baſe pourroit être prolongée du côté du nord d'environ 3 à 4000 pieds; en rapprochant un peu vers l'eſt ſon extrémité ſeptentrionale, en lui donnant la direction *b β*; elle ſeroit alors encore plus favorablement diſpoſée pour déterminer la ſtation *e*, d'où, juſques à la ſtation *d*, très bien déterminée par la même baſe ſur la montagne de Choutagne, la diſtance ſeroit d'environ 7500 toiſes, et l'angle oppoſé en *f* ſur la montagne de la Caille d'environ 34 degrés, c'eſt-à-dire double du précédent. Cette diſpoſition du premier triangle de la chaîne ſeroit ſans contredit plus avantageuſe que la précédente.

Le mont Colombier eſt élevé d'environ 650 toiſes * au-deſſus du niveau de la plaine; les points *c* et *d* ſur la montagne de Choutagne ſont élevés d'environ 380 toiſes, et la montagne de la Caille d'environ 200 toiſes ſeulement; c'eſt la plus baſſe de toutes celles qui ſervent à des ſtations; mais comme elle eſt tout-à-fait iſolée, elle eſt viſible de tous les côtés à une diſtance ſuffiſante.

Depuis les points *f* et *d* on détermine la ſtation *g* ſur la Tournette; et ces trois points forment le triangle central d'où l'on procède enſuite au nord et au ſud dans la direction de la méridienne. La Tournette eſt une montagne élevée d'environ 1000 toiſes au-deſſus du Lac de Genève; elle eſt iſolée, et offre un des points de vûe les plus frappans et les plus étendus qu'on puiſſe trouver dans ces régions. On apperçoit très bien depuis ce ſommet non ſeulement les deux extrémités de l'arc du méridien, mais encore preſque toutes les ſtations, excepté celles qui ſont dans le Vallais au-delà du glacier de Buët ou de la Mortine. Le ſommet de la montagne d'où j'ai obſervé mes angles n'eſt pas du plus facile accès; mais j'ái remarqué à environ 150 toiſes plus bas un endroit très acceſſible, nommé le Plan du Rivray, qui ſeroit convenable pour la ſtation. L'inſpection de la carte montre combien cette ſtation eſt importante, puiſqu'elle n'offre pas moins de ſept points à obſerver.

En partant du triangle central *dfg* pour aller au midi, on détermine par les ſtations *d* et *g* la ſtation *h* ſur la Dent de Nivolet. Cette montagne eſt très voiſine de Chambery, capitale le la Savoye; et quoique fort eſcarpée à l'oueſt, elle eſt, m'a--on dit, d'un facile accès du côté de l'eſt.

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* Les hauteurs de la plupart des ſtations ne ſont indiquées qu'à-peu-près, et pour donner une idée des directions reſpectives des rayons viſuels rélativement à l'horiſon, et de la néceſſité d'avoir un inſtrument dont le mouvement dans les plans verticaux ſoit de la plus extrême préciſion.

Il exiſte vers le bord occidental du Lac du Bourget une montagne appellée la Dent du Chat, ſur laquelle je ſuis monté, et dont j'ai déterminé la poſition au moyen d'une baſe qui eſt déſignée ſur la carte. Cette montagne eſt élevée d'environ 650 toiſes au-deſſus du Lac du Bourget, et acceſſible, quoiqu'avec quelque difficulté; elle offriroit une ſtation ſubſidiaire, dans le cas où celle ſur la Dent de Nivolet ſeroit impraticable, ce qui n'eſt cependant nullement probable. La poſition de cette dernière ſeroit d'ailleurs infiniment meilleure rélativement à la ſuite de la chaîne.

Avec les ſtations *g* et *h* on détermine le point *i* ſur une montagne que j'ai vûe depuis la Tournette, qui m'a paru très acceſſible, et qui termine la chaîne qui accompagne la rive droite de la rivière d'Arc depuis St. Jean de Maurienne. J'ignore le nom de cette montagne; et c'eſt ici que commence la partie de ma carte qui n'offre que des probabilités.

Des ſtations *h* et *i* on détermine le point *k*, où les cartes indiquent une montagne qu'elles nomment Coce*iron*. Cette montagne, formant l'extrémité d'une chaîne entre deux vallées dans la direction des deux ſtations *i* et *h*, doit être viſible depuis l'une et l'autre. C'eſt aux environs de cet endroit que j'ai lieu de préſumer que la méridienne de l'Obſervatoire de Genève, prolongée au ſud depuis le ſommet de la montagne de Salève, vient paſſer, ſans être interceptée dans l'intervalle par aucune des montagnes qui s'y rencontrent.

Avec les ſtations *i* et *k* on détermine la ſtation *l* ſur la chaîne qui ſépare la Maurienne de la Tarentaiſe, et dans laquelle j'ai vû pluſieurs ſommités acceſſibles: et enfin avec les ſtations *k* et *l* on détermine la ſtation *m* ſur le mont Sapey, où j'ai été, et d'où j'ai vû la direction générale des chaînes telle qu'elle eſt indiquée ſur la carte; et en particulier que le rayon viſuel *m k* eſt parfaitement libre. Du mont Sapey et du point *l*, choiſi convenablement, on détermine finalement St. Jean de Maurienne.

J'ai obſervé en montant le Sâpey une très haute montagne inacceſſible, ſituée au midi de St. Jean de Maurienne, et dont le ſommet offre trois aiguilles contigues de la forme que j'ai deſſinée ſur la carte, et qui m'ont paru faciles à reconnoître de loin. Ces aiguilles, que j'ai très bien retrouvées enſuite depuis la Tournette, ſe nomment, à ce qu'on m'a dit, les Aiguilles d'Arve; et elles m'ont ſervi à déterminer à-peu-près la poſition de la ville de St. Jean rélativement à la méridienne de Genève; du moins à m'aſſurer qu'elle ne s'écarte pas beaucoup de cette méridienne. Je ne repéterai pas ici ce que j'ai dit dans le Mémoire ſur la convenance que je vois à la choiſir pour terme méridional de l'arc à meſurer.

Je reprends le triangle central *dfg* pour marcher de-là vers le nord de la chaîne des triangles.

Les ſtations *f* et *g* déterminent la ſtation *o* ſur le Pithon de Salève. C'eſt un rocher iſolé et du plus facile accès, qui domine la montagne; il eſt élevé d'environ 512 toiſes au-deſſus du Lac de Genève, et parfaitement viſible depuis l'Obſervatoire de cette ville.

Avec les ſtations *g* et *o* on obtient la ſtation *p* ſur le Môle. Cette montagne eſt élevée d'environ 750 toiſes au-deſſus du Lac de Genève; elle eſt de forme conique, parfaitement iſolée, d'un facile accès, et offre une des plus belles ſtations de toute la meſure; elle préſente d'autres avantages pour des expériences ſur l'attraction latérale, pour des obſervations barométriques et météorologiques; en un mot, elle mérite toute l'attention des obſervateurs, auxquels elle offrira d'ailleurs une ſtation auſſi importante que celle de la Tournette.

Avec les ſtations *o* et *p* ſur le Pithon et ſur le Môle, on arrive à l'Obſervatoire de Genève, ſur lequel j'ai donné dans le Mémoire des détails qui me paroiſſent ſuffiſans; je le déſigne par la lettre *q*.

Avec les ſtations *o* et *q* on détermine un point quelconque *r*, acceſſible et habitable dans la chaîne du Mont Jura, aux environs du parallèle de l'Obſervatoire de Genève, et qui forme le terme occidental de la meſure; on a à cet égard beaucoup de choix; le glacier de Buet, ſur lequel ſe fera le ſignal, eſt viſible de tous les points du Jura près du parallèle de Genève. On trouve des habitations juſques vers le ſommet de cette derniere montagne, et on choiſira la plus convenable pour y établir l'obſervateur et ſa pendule.

La ligne *q p*, menée de l'Obſervatoire au Môle, ſert de baſe au triangle *qps*, dont le ſommet *s* eſt à Vincy, village du Pays de Vaud, viſible de l'Obſervatoire de Genève, et dans lequel j'ai une maiſon de campagne, que j'ai offerte dans mon Mémoire pour y faire les obſervations aſtronomiques dans le cas où l'on jugeroit à-propos d'étendre juſques là l'arc du méridien. Cet endroit eſt élevé d'environ 80 toiſes au-deſſus du Lac, et ſeroit commode pour les obſervateurs.

Ce triangle *qps* qui appartient à la méridienne, eſt auſſi le premier de la ſuite deſtinée à déterminer la longueur du parallèle à l'eſt. La ligne *p s*, menée du Môle à Vincy, et les ſtations *p* et *s*, déterminent l'une ou l'autre des Montagnes du Chablais appellées la Dent d'Oche et la Cornette. Elles ſont voiſines l'une de l'autre, et je les déſigne toutes deux, parce que la Dent d'Oche ſur laquelle je ſuis monté eſt d'un aſſez difficile accès; la Cornette ſur laquelle je n'ai point été eſt, dit-on, très facile à monter, et a de plus l'avantage d'être un peu plus haute que la Dent d'Oche (qui eſt cependant élevée de 940 toiſes au deſſus du lac), et un peu plus à l'eſt du côté des montagnes du Vallais. Je préférerois donc la ſtation de la Cornette telle que je l'ai indiquée.

La ligne *tp* de la Cornette au Môle ſert de baſe au triangle *ptv*, dont le ſommet eſt à la Dent de Vaulion, montagne dans la chaîne de Jura, juſqu'où je propoſe d'étendre encore, ſi l'on veut, l'arc du méridien.

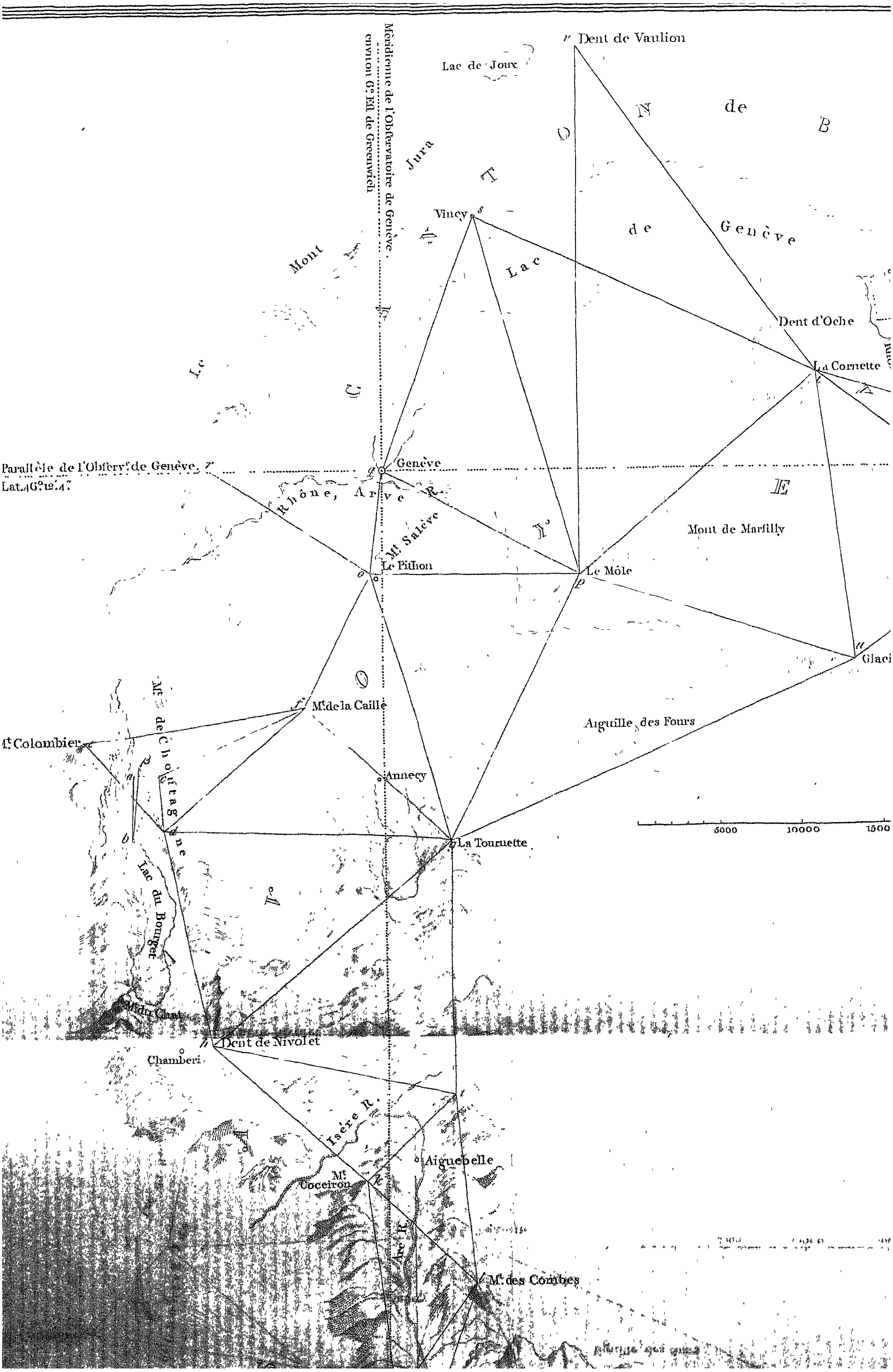
Cette même ligne *tp* ſert de baſe à un triangle *ptu*, dont le ſommet *u* eſt au glacier de Buët, qui ſera à la fois une ſtation pour meſurer des angles, et le lieu du ſignal pour les obſervations rélatives à la meſure aſtronomique de l'arc du parallèle. Cette montagne, élevée d'environ 1300 toiſes au-deſſus du Lac de Genève, eſt couverte d'une couche de neige permanente; mais cela n'empêcheroit pas qu'on ne pût y faire commodément les obſervations dans les mois d'Août ou de Septembre. On peut bâtir une petite cabane pour le ſéjour néceſſaire, ſur des rochers d'ardoiſe qui ſont voiſins de la ſommité. La montagne n'eſt pas d'un accès difficile par la route que M. EXCHAQUET a le premier indiquée, et que j'ai ſuivie à mon dernier voyage à cette montagne; et lors même qu'il reſteroit quelques difficultés à vaincre, cette ſtation eſt ſi belle et ſi importante, qu'on ne manqueroit pas de motifs et de courage pour les ſurmonter.

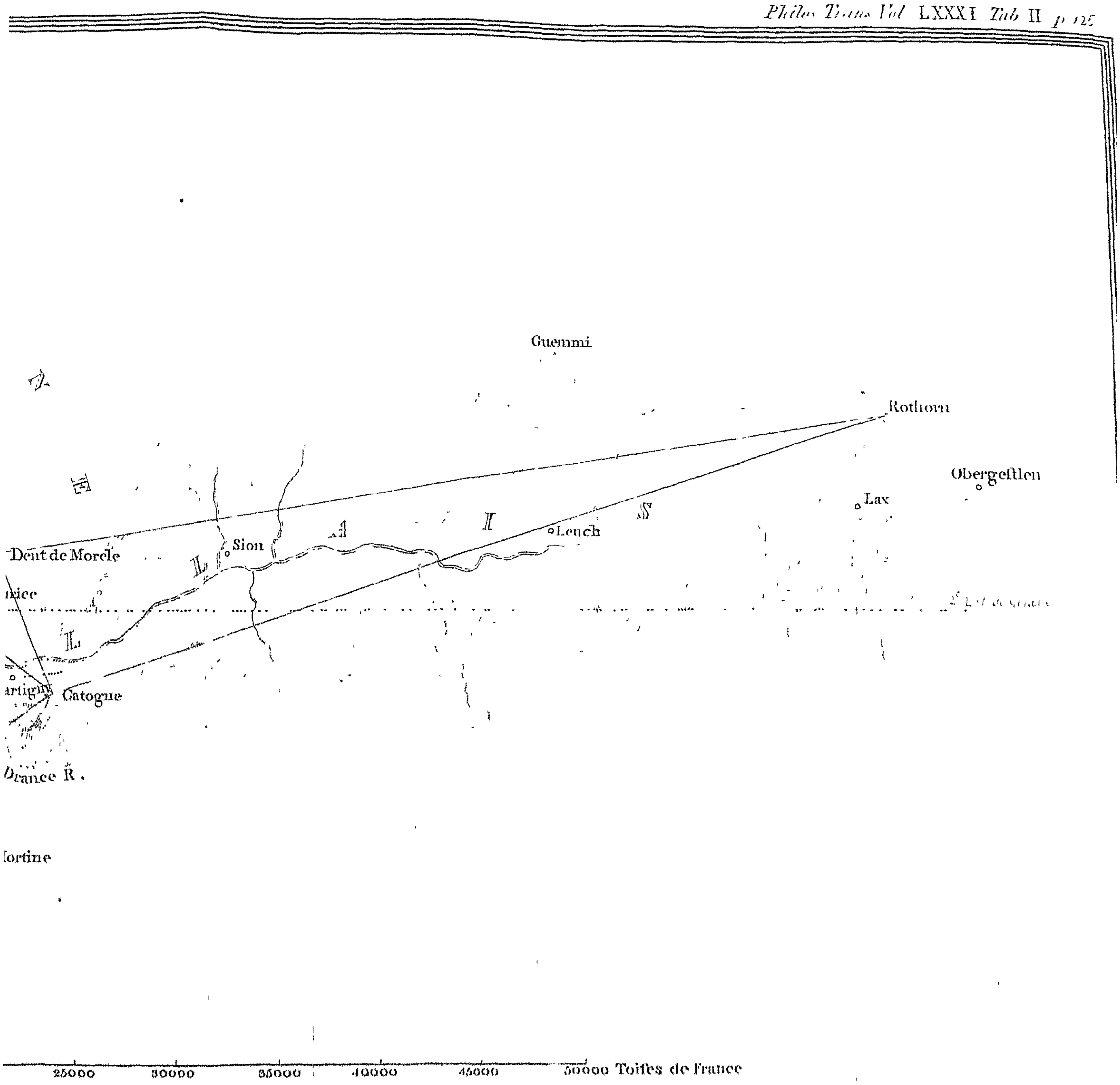
On remarquera qu'il ſe forme ainſi autour du Môle un exagone irrégulier, ce qui fournit l'occaſion d'une vérification; et qu'entre autres, la ligne *pu* peut être déterminée de deux manieres; ſavoir, immédiatement par le triangle *pgu*, et en faiſaint le circuit que nous venons de faire par les ſtations *goqst*.

Ici commence la partie de ma carte que je dois à la complaiſance de mon ami M. WILD. Il me prévient que les poſitions qu'il m'a déſignées ne ſont pas exactes; mais il ne s'agiſſoit que d'établir la direction générale du haut et bas Vallais, de marquer les principales montagnes acceſſibles, et les endroits où l'on pourroit meſurer des baſes de vérification. La carte du Vallais qu'il m'a envoyée, et dont la mienne offre l'extrait, m'a paru atteindre ce but. Il eſt évident que le triangle qui a pour baſe la diſtance de la Dent de Morcles à la Catogne, et qui ſe termine au Rothorn, non loin de la ſource du Rhône, auroit l'angle du ſommet beaucoup trop aigu; mais il n'eſt pas douteux auſſi que dans une vallée large et preſque droite, comme l'eſt celle du haut Vallais, on peut former facilement une ſuite de triangles par des ſtations alternatives ſur les ſommets acceſſibles des deux côtés de la vallée, et arriver à l'extrémité par un petit nombre de ces triangles.

Les lignes droites, tracées en rouge dans cette partie de la carte, déſignent les baſes praticables; ces baſes ſont au nombre de quatre, mais aucune d'elles n'égale en longueur celle que j'ai trouvée dans les plaines de Choutagne.

Si le projet dont je viens de développer l'eſquiſſe, eſt agréé par la Société Royale, mon premier ſoin, dès que la ſaiſon le permettra, ſera de parcourir le haut Vallais pour découvrir le dernier ſommet acceſſible du coté de l'eſt, d'où la Mortine ou le glacier de Buet ſoit viſible. Ce point une fois trouvé, j'établirai


Méridienne de l'Obſervatoire de Genève.
environ 6°. Eſt de Greenwich
Dent de Vaulion
Lac de Joux
Jura
Mont
Le
Vincy
T
O
N
de
B
Lac
de
Genève
Dent d'Oche
La Cornette
Parallèle de l'Obſerv.e de Genève, z
Lat. 46°. 12'. 4"
Genève
Rhône, Arve R.
Mt. Salève
Le Pithon
Le Môle
Mont de Marſilly
E
Glaci
Mt. de la Caille
Aiguille des Fours
Mt. Colombier
Mt. de Chouftagne
Annecy
La Tournette
5000
10000
1500
Lac du Bourget
Mt. du Chat
Dent de Nivolet
Chamberi
Isère R.
Aiguebelle
Mt. Coceiron
Arc R.
Mt. des Combes


Guemmi
Rothorn
Obergeſtlen
Lax
Leuch
Sion
Dent de Morcle
rice
artigny
Catogne
Drance R.
Iortine
25000
30000
35000
40000
45000
50000 Toiſes de France

j'établirai la ſuite de triangles pour y parvenir; et l'incertitude qui me reſte ſur l'étendue totale de l'arc du parallèle dont les deux extrémités ſont viſibles depuis la montagne dont je viens de parler, ſera levée.

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BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL.

for January 1790.

1790	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	34	53	29,92		W	1	Fine.
	2	0	40	56,5	30,08		W	1	Fair.
2	8	0	36	53,5	30,37		ENE	1	Cloudy.
	2	0	42	55	30,32		S by E	1	Cloudy.
3	8	0	44	53	30,01	0,093	WSW	1	Cloudy.
	2	0	50	55,5	30,00		WNW	1	Cloudy.
4	8	0	42	53,5	30,25				Foggy.
	2	0	45	55	30,26		NW	1	Cloudy.
5	8	0	43	53	30,26				Foggy.
	2	0	45	55	30,24		ENE	1	Cloudy.
6	8	0	40,5	52,5	30,20		E	1	Cloudy.
	2	0	40,5	53,5	30,22		SSE	1	Cloudy.
7	8	0	40	51,5	30,42		WNW	1	Cloudy.
	2	0	41,5	54	30,47		WNW	1	Hazy.
8	8	0	33,5	51	30,44		WNW	1	Foggy.
	2	0	35	53	30,40		WNW	1	Cloudy.
9	8	0	32	50	30,30				Foggy.
	2	0	39	53	30,25		SSE	1	Fine.
10	8	0	33,5	50	30,24		SW	1	Foggy.
	2	0	40	53	30,22		W	1	Cloudy.
11	8	0	44	50,5	30,13		WSW	1	Cloudy.
	2	0	47	53,5	30,10		WSW	1	Cloudy.
12	8	0	49,5	52,5	29,88	0,025	WSW	2	Cloudy.
	2	0	52,5	56,5	29,86		WSW	2	Cloudy.
13	8	0	50,5	54	29,88		WSW	2	Cloudy.
	2	0	48,5	55	29,86		WSW	2	Rain.
14	8	0	46	53	29,86	0,073	NW	1	Cloudy.
	2	0	47	56	29,90		NNE	1	Cloudy.
15	8	0	41,5	53	29,94	0,030	N	1	Cloudy.
	2	0	40	56	29,99		W by N	1	Cloudy.
16	8	0	40	53	30,01	0,137	WNW	2	Cloudy.
	2	0	45	55	30,11		WNW	2	Cloudy.

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1790.	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
			°	°	Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	38,5	53	30,39		NW	1	Cloudy.
	2	0	42	55	30,39		NNE	1	Cloudy.
18	8	0	38	52	30,22		ENE	2	Cloudy.
	2	0	40	53,5	30,12		ENE	2	Fine.
19	8	0	33	50	30,14		E	2	Cloudy.
	2	0	38	52,5	30,16		E	2	Fine.
20	8	0	32	49,5	30,25		E	2	Fine.
	2	0	35	51,5	30,31		E	2	Fine.
21	8	0	32	50	30,44		E	1	Fair.
	2	0	39	53	30,41		E	1	Fine.
22	8	0	33	50,5	30,38		E	1	Foggy.
	2	0	38	52,5	30,37		WNW	1	Cloudy.
23	8	0	42	52	30,28		WNW	1	Cloudy.
	2	0	47	54	30,28		WNW	1	Cloudy.
24	8	0	43	52	30,20		WNW	1	Cloudy.
	2	0	47	54,5	30,17		WNW	1	Cloudy.
25	8	0	42	52,5	30,11	0,250	NNE	1	Rain.
	2	0	43,5	55	30,20		N	1	Fair.
26	8	0	35,5	50,5	30,22		WNW	1	Cloudy.
	2	0	41	52,5	30,11		W	1	Fair.
27	8	0	39,5	51,5	29,35	0,181	WNW	2	Fair.
	2	0	42	55	29,35		WNW	2	Fair.
28	8	0	39	52	29 56		W	1	Cloudy.
	2	0	48	54,5	29,27		SW	2	Cloudy.
29	8	0	37	52,5	29,28	0,160	WNW	2	Fine.
	2	0	41,5	54,5	29,28		WNW	2	Fine.
30	8	0	38	52,5	29,51		NW	2	Cloudy.
	2	0	42,5	54,5	29,69		NW	1	Fine.
31	8	0	38,5	52,5	29,62	0,018	WNW	1	Cloudy.
	2	0	44	54,5	29,63		WNW	1	Fine.

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1790	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H	M.	°	°	Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	37	52	29,88		WNW	1	Fine.
	2	0	43	56	30,02		NW	1	Fine.
2	8	0	37	52	30,20		WNW	1	Fair.
	2	0	45,5	54	30,24		W	1	Fair.
3	8	0	43	52,5	30,38		W	1	Cloudy.
	2	0	47	55	30,43		W	1	Cloudy.
4	8	0	44	53	30,61		W	1	Cloudy.
	2	0	46	54	30,62		W	1	Cloudy.
5	8	0	43,5	52	30,61		W	1	Cloudy.
	2	0	46,5	54.5	30,58		W	1	Cloudy.
6	8	0	42	53	30,51		WNW	1	Fair.
	2	0	48,5	56	30,51		WNW	1	Fine.
7	8	0	42	53	30,45		WNW	1	Cloudy.
	2	0	42,5	56	30,37		WNW	1	Cloudy.
8	8	0	38	52,5	30,25		NW	1	Cloudy.
	2	0	39,5	53,5	30,22		WNW	1	Cloudy.
9	8	0	40	52,5	30,08		WNW	1	Cloudy.
	2	0	43	53	30,03		WNW	1	Cloudy.
10	8	0	42,5	53	30,07		NE	1	Cloudy.
	2	0	43	54	30,25		E	1	Cloudy.
11	8	0	39	52,5	30,22		WSW	1	Cloudy.
	2	0	49	55	30,22		W	1	Cloudy.
12	8	0	44	53	30,13		W	1	Cloudy.
	2	0	50	57	30,17		W	1	Fair.
13	8	0	40,5	54	30,42		W	1	Fine.
	2	0	49	58	30.34		W	1	Cloudy.
14	8	0	42	54	30,18		WSW	1	Cloudy.
	2	0	48	56	30,10		WSW	1	Cloudy.
15	8	0	36	53	30,24		W	1	Fine.
	2	0	45	56	30,23		W	1	Fine.
16	8	0	43 5	53,5	30,00		S by W	2	Cloudy.
	2	0	44	56	29,95		WSW	2	Cloudy.

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1790	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str	
Feb. 17.	8	0	35	54	30,22		WNW	1	Fine.
	2	0	46	56,5	30,26		WNW	1	Fair.
18	8	0	38	54	30,43		WNW	1	Foggy.
	2	0	46	57	30,45		WNW	1	Fine.
19	8	0	40	54	30,49		SW	1	Cloudy.
	2	0	47	58	30,46		SW	1	Fine.
20	8	0	35,5	53,5	30,44		N	1	Foggy.
	2	0	44	57	30,43		N	1	Hazy.
21	8	0	33	53	30,40				Foggy.
	2	0	40,5	57	30,38		NE	1	Hazy.
22	8	0	37	53	30,22		NE	1	Fine.
	2	0	49	57,5	30,15		S by W	1	Fine.
23	8	0	45,5	55	30,00	0,115	E	1	Cloudy.
	2	0	50,5	57	30,00		WNW	1	Cloudy.
24	8	0	44	55	30,17		WNW	1	Cloudy.
	2	0	50	57	30,17		SW	1	Cloudy.
25	8	0	49	55,5	30,04		SW	1	Cloudy.
	2	0	55	58	30,11		WSW	1	Cloudy.
26	8	0	49	56	30,02		SW	2	Cloudy.
	2	0	53	58,5	29,88		SW	2	Cloudy.
27	8	0	40,5	56	30,23		WNW	2	Fine.
	2	0	50	59,5	30,29		WNW	2	Fair.
28	8	0	42,5	56	30,25		W	1	Cloudy.
	2	0	50	59,5	30,25		WNW	1	Fair.

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1790	Time.		Therm. without	Therm within	Barom.	Rain.	Winds.		Weather.
	H.	M	°	°	Inches.	Inch.	Points.	Str	
Mar. 1	7	0	46	56	30,35		NW	1	Cloudy.
	2	0	50	58	30,38		NW	1	Cloudy.
2	7	0	46,5	56	30,32		W	1	Cloudy.
	2	0	53	58	30,38		NNW	1	Cloudy.
3	7	0	47	56	30,38		NW	1	Cloudy.
	2	0	54	59	30,39		NW	1	Fair.
4	7	0	40	55,5	30,28		NW	1	Fine.
	2	0	49	59	30,27		NW	1	Fine.
5	7	0	40	56	30,32		NNW	1	Cloudy.
	2	0	46	59	30,39		NNW	1	Fair.
6	7	0	36	55	30,46		NE	1	Fair.
	2	0	44	57	30,45		NW	1	Fair.
7	7	0	40	54	30,44		NE	1	Cloudy.
	2	0	46	57	30,44		N	1	Cloudy.
8	7	0	41	54	30,44		ENE	1	Cloudy.
	2	0	47	55,5	30,42		S	1	Cloudy.
9	7	0	39	53	30,33		W	1	Cloudy.
	2	0	50,5	55,5	30,18		W	2	Cloudy.
10	7	0	43	33,5	29,88		W	2	Fair.
	2	0	51	56	29,91		W	2	Fair.
11	7	0	37	53,5	30,24		W	2	Fine.
	2	0	51	56	30,30		W	2	Cloudy.
12	7	0	48	54	30,30	0,042	W	2	Cloudy.
	2	0	56	58	30,32		W	2	Fair.
13	7	0	50,5	56,5	30,35		W	2	Cloudy.
	2	0	56	58,5	30,38		W	2	Cloudy.
14	7	0	38	55	30,55	0,080	W	1	Fine.
	2	0	50	56	30,52		NW	1	Fine.
15	7	0	35	54	30,59		NNW	1	Fine.
	2	0	46	55,5	30,62		N	1	Cloudy.
16	7	0	35	53,5	30,65		NNE	1	Fair.
	2	0	46	57	30,62		NNE	1	Fair.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	33	52,5	30,63		NE	1	Fair.
	2	0	43,5	56,5	30,63		E	1	Fine.
18	7	0	38	53	30 00		NE	1	Cloudy.
	2	0	47	57	30,56		NE	1	Fair.
19	7	0	42	54	30,45		NNE	1	Cloudy.
	2	0	49	57,5	30,44		NE	1	Fine.
20	7	0	38	54,5	30,42		E	2	Fine.
	2	0	48	57	30,42		E	1	Fine.
21	7	0	34,5	54	30,40		NE	2	Fine.
	2	0	49	59	30,33		E	2	Fine.
22	7	0	38,5	53,5	30,17		ENE	1	Fine.
	2	0	51,5	58,5	30,06		E	2	Fine.
23	7	0	40,5	54	29,85		E	1	Cloudy.
	2	0	50	57	29,85		ENE	1	Cloudy.
24	7	0	47,5	54	29,83		E	1	Cloudy.
	2	0	52	59	29,84		SW	1	Cloudy.
25	7	0	42	55	30 02		WSW	1	Cloudy.
	2	0	54	59	30,06		SW	1	Fair.
26	7	0	43	56	30,06		E	1	Cloudy.
	2	0	53	59	30,05		E	1	Fair.
27	7	0	43	55,5	30,01		E	1	Cloudy.
	2	0	53	59	30,00		E	1	Fair.
28	7	0	41	55,5	30,00		ENE	1	Cloudy.
	2	0	53	58	30,00		ENE	1	Fair.
29	7	0	42	56	30,00		ENE	1	Cloudy.
	2	0	48	58	30.00		NE	1	Cloudy.
30	7	0	42	56	30,00		NE	1	Cloudy.
	2	0	48	58	29,99		ENE	1	Cloudy.
31	7	0	41	55	29,96		NE	1	Fair.
	2	0	52	60	29,96		E	1	Fine.

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1790	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
April 1	7	0	42	55	30,07		E	2	Fair.
	2	0	42,5	57	30,08		E	2	Fair.
2	7	0	37	52	30,25		E	2	Fine.
	2	0	43	57	30,26		E	2	Fine.
3	7	0	35	53,5	30,30		N	2	Fine.
	2	0	48	58	30,25		NE	2	Fine.
4	7	0	39	53,5	30,23		NE	2	Fine.
	2	0	49	58,5	30,22		E	2	Fine.
5	7	0	37	54	30,26		NNE	2	Fair.
	2	0	47,5	58	30,22		E	2	Cloudy.
6	7	0	40	53,5	30,08		E	2	Cloudy.
	2	0	42	59	30,00		E	2	Fair.
7	7	0	38	53	29,80		NNE	2	Fair.
	2	0	47	59	29,74		E	2	Fine.
8	7	0	43	55	29,69		E	2	Fair.
	2	0	50	58,5	29,71		E	2	Fine.
9	7	0	40	55	29,62		E	2	Cloudy.
	2	0	47	57	29,55		E	2	Cloudy.
10	7	0	36	53,5	29,46	0,173	ENE	2	Rain.
	2	0	39,5	56	29,48		NE	2	Cloudy.
11	7	0	37	53	29,38	0,024	NE	2	Cloudy.
	2	0	38,5	55	29,44		NNE	2	Snow.
12	7	0	37	53	29,40	0,054	N	2	Cloudy.
	2	0	43	54,5	29,48		N	2	Cloudy.
13	7	0	38	52,5	29,61		N	2	Cloudy.
	2	0	40,5	55	29,66		NNE	2	Rain.
14	7	0	37	52	29,82	0,140	NW	1	Cloudy.
	2	0	42,5	56	29,86		NW	2	Cloudy.
15	7	0	36	53	29,76		ENE	1	Rain.
	2	0	42,5	55,5	29,45		ENE	1	Rain.
16	7	0	38	52	29,72	0,593	N	1	Cloudy.
	2	0	43	55	29,87		N	1	Cloudy.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
April 17	7	0	35	52	29,98		NNW	1	Fine.
	2	0	46	56	29,96		N	1	Fair.
18	7	0	35	52	30,01		N	1	Fine.
	2	0	46	56	30,02		E	2	Cloudy.
19	7	0	35,5	52,5	30,05		N	2	Fine.
	2	0	45	55,5	30,05		N	2	Cloudy.
20	7	0	39	52,5	30,29		E	1	Cloudy.
	2	0	46	56	30,29		E	1	Fine.
21	7	0	39	52	30,24		E	1	Fine.
	2	0	50	55	30,20		S	1	Cloudy.
22	7	0	46	51,5	29,96		E by S	1	Cloudy.
	2	0	55	55,5	29,84		S	1	Fair.
23	7	0	51	54,5	29,72		WSW	1	Fair.
	2	0	58	59	29,66		WSW	2	Cloudy.
24	7	0	48	56	29,49	0,200	SW by W	1	Cloudy.
	2	0	53	59	29,50		WSW	1	Cloudy.
25	7	0	45	56	29,66	9,035	WSW	1	Cloudy.
	2	0	54	59	29,63		WSW	1	Cloudy.
26	7	0	44	56	29,73	0,025	WSW	1	Cloudy.
	2	0	54	59,5	29,80		SW	1	Rain.
27	7	0	42	57	29,98	0,201	N	1	Cloudy.
	2	0	55	59,5	30,04		N	1	Cloudy.
28	7	0	41	56	30,10		NW	1	Fine.
	2	0	55	59,5	30,02		SW	1	Cloudy.
29	7	0	48,5	57	29,83		SSW	2	Cloudy.
	2	0	54	58,5	29,73		SSW	2	Cloudy.
30	7	0	47	57	29,52	0,025	SW	2	Fair.
	2	0	56	59,5	29,62		SW	2	Fair.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
May 1	7	0	48	58	29,63		S Eby E	1	Cloudy.
	2	0	51	60,5	29,55		E	1	Rain.
2	7	0	48	58,5	29,50	0,175	SSW	2	Cloudy.
	2	0	57,5	61	29,61		SSW	2	Fair.
3	7	0	47	58	29,86		WSW	1	Fair.
	2	0	59,5	61	29,87		NW	1	Fair.
4	7	0	47	58	30,04		W	1	Fine.
	2	0	62	61,5	30,04		SW	1	Fine.
5	7	0	52,5	58,5	29,92		W	1	Cloudy.
	2	0	55	62	29,88		NNW	1	Cloudy.
6	7	0	48	58,5	29,75	0,110	W	1	Rain.
	2	0	51	62	29,72		N	1	Rain.
7	7	0	49	60	29,68	0,280	WNW	1	Cloudy.
	2	0	57	63	29,74		E	1	Cloudy.
8	7	0	51	60,5	29,83		N	1	Cloudy.
	2	0	60,5	63,5	29,83		NNE	1	Fair.
9	7	0	52	61	29,94		N	1	Fine.
	2	0	59,5	64	29,98		N	1	Fair.
10	7	0	51,5	62	30,01	0,086	N	1	Cloudy.
	2	0	58	62	30,02		E	1	Cloudy.
11	7	0	51	62	30,06		N	1	Cloudy.
	2	0	64	64	30,06		N	1	Fair.
12	7	0	52	61,5	30,09		NE	1	Fine.
	2	0	62	66	30,09		N	2	Fine.
13	7	0	50	61	30,14		NE	2	Fine.
	2	0	63,5	64,5	30,11		NE	2	Fine.
14	7	0	50	61	30,10		NE	1	Fine.
	2	0	64	66	30,09		N	1	Fine.
15	7	0	50,5	61,5	30,03		NW	1	Hazy.
	2	0	61,5	65	29,99		WNW	1	Fine.
16	7	0	54	62	29,91		ESE	1	Fine.
	2	0	66	64	29,86		E	2	Fine.

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for May 1790.

1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
May 17	7	0	56	62,5	29,68		SW	2	Fine.
	2	0	57	64,5	29,66		WSW	2	Fair.
18	7	0	51	57	29,88	0,130	W	1	Fine.
	2	0	60	60	29,88		W	2	Cloudy.
19	7	0	53	59	29,76		S	2	Fine.
	2	0	63	60	29,69		S	2	Cloudy.
20	7	0	50	59	29,77	0,131	WSW	2	Fine.
	2	0	59,5	60	29,77		WSW	2	Cloudy.
21	7	0	50	58,5	29,96		W	1	Fine.
	2	0	59,5	59,5	29,96		W	1	Cloudy.
22	7	0	50,5	58,5	29,94	0,033	S	1	Cloudy.
	2	0	57	59	29,86		SE	1	Cloudy.
23	7	0	56	58,5	29,80	0,190	WSW	1	Cloudy.
	2	0	64	60	29,84		S	1	Cloudy.
24	7	0	54	60	29,80	0,201	E	1	Rain.
	2	0	63	61	29,72		S by W	2	Cloudy.
25	7	0	53	60,5	29,85	0,200	S	1	Rain.
	2	0	61	60,5	29,85		NE	1	Cloudy.
26	7	0	54	61	29,94	0,011	E	1	Cloudy.
	2	0	64	61	29,98		E	1	Cloudy.
27	7	0	55	61	30,00	0,223	NW	1	Cloudy.
	2	0	64	62,5	29,96		WNW	1	Fair.
28	7	0	58	62	30,00	0,881	NE	1	Cloudy.
	2	0	66	63,5	29,98		ENE	1	Fair.
29	7	0	58	62,5	29,99	0,137	E	1	Cloudy.
	2	0	63	63	30,02		S	1	Cloudy.
30	7	0	58	63	30,05		W	1	Cloudy.
	2	0	63	63	30,04		W	1	Cloudy.
31	7	0	56	62	29,97	0,110	WNW	1	Cloudy.
	2	0	63	63	30,02		N	1	Cloudy.

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for June 1790.

1790	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
June 1	7	0	56	62	30,17	0,022	W	1	Fine.
	2	0	66	62,5	30,12		SW	1	Fair.
2	7	0	60	62,5	30,16		N	1	Fair.
	2	0	66,5	64,5	30,16		N	1	Fair.
3	7	0	59	63	30,16		W	1	Cloudy.
	2	0	64	63	30,07		W	1	Cloudy.
4	7	0	59,5	63	30,07		W	1	Fair.
	2	0	65	64,5	30,08		NW	1	Fair.
5	7	0	55	62	30,20		WNW	1	Fair.
	2	0	63	63,5	30,16		WNW	1	Fair.
6	7	0	56,5	61,5	30,17		W	1	Fine.
	2	0	63	62	30,16		WNW	1	Fair.
7	7	0	57	61	29,96	0,040	WNW	1	Cloudy.
	2	0	65	63	29,96		WNW	1	Fair.
8	7	0	56	61	29,92		WSW	2	Fine.
	2	0	63	61,5	29,79		SSW	2	Cloudy.
9	7	0	58	61	29,56	0,060	SSW	2	Fair.
	2	0	63	61,5	29,49		SSW	2	Fair.
10	7	0	57	60	29,58		W	2	Fair.
	2	0	60	61	29,66		S	1	Rain.
11	7	0	53	60,5	29 89	0,340	NNW	1	Fair.
	2	0	64	62	29,85		WNW	1	Fair.
12	7	0	55	60,5	29,98		NW	1	Cloudy.
	2	0	62	62	30,02		NNW	1	Fair.
13	7	0	53	60	30,26		N	1	Fine.
	2	0	61	62	30,27		N	1	Fair.
14	7	0	56	60	30,33				Fine.
	2	0	66	62	30,30				Fine.
15	7	0	61	61,5	30,29		N	1	Fair.
	2	0	67	63,5	30,28		E	1	Cloudy.
16	7	0	54	61,5	30,24		NE	1	Cloudy.
	2	0	63	62	30,19		NE	1	Cloudy.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
June 17	7	0	58	61	30,08		NE	1	Cloudy.
	2	0	65	62,5	30,01		E	1	Cloudy.
18	7	0	58	61	29,88	0,051	W	1	Fair.
	2	0	68	63	29,94		W	2	Cloudy.
19	7	0	59,5	62	30,00		SW	2	Cloudy.
	2	0	69	64	30,01		SW	2	Cloudy.
20	7	0	60	62,5	30,23		WSW	1	Fair.
	2	0	69,5	65	30,28		WSW	2	Fine.
21	7	0	65	65	30,35		WSW	1	Fine.
	2	0	74	67,5	30,35		WSW	1	Fine.
22	7	0	69	67	30,20				Fine.
	2	0	86	73,5	30,10		SSW	1	Fine.
23	7	0	71	71	29,96		W	1	Cloudy.
	2	0	76	72	29,96		W	1	Cloudy.
24	7	0	60	66	29,98		W	2	Fair.
	2	0	68	68	29,91		W	2	Cloudy.
25	7	0	59	65	29,87		WSW	2	Fair.
	2	0	68	67	29,87		WSW	2	Fair.
26	7	0	57	65	29,89		W	2	Cloudy.
	2	0	66,5	66,5	29,89		WNW	2	Fair.
27	7	0	53	64,5	29,96	0,120	NW	1	Fair.
	2	0	64	66	30,02		NW	1	Cloudy.
28	7	0	53	63	30,08	0,075	WNW	1	Cloudy.
	2	0	64	65	30,07		W	1	Fair.
29	7	0	55	63	30,02		SW	1	Cloudy.
	2	0	69,5	63,5	29,97		SSE	1	Cloudy.
30	7	0	53	63	29,93		SW	1	Cloudy.
	2	0	68	64	29,91		SW	2	Cloudy.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M	°	°	Inches.	Inch.	Points.	Str.	
July 1	7	0	60	63,5	29,82	0,048	WSW	2	Cloudy.
	2	0	68	64	29,83		WSW	2	Fair.
2	7	0	56,5	63	29,72		WSW	1	Fair.
	2	0	63	63,5	29,63		SW	1	Rain.
3	7	0	54,5	63	29,68	0,088	W	1	Fine.
	2	0	70	64	29,68		WNW	1	Cloudy.
4	7	0	55	63	29,70		SW	1	Fair.
	2	0	66	64	29,63		SSW	2	Cloudy.
5	7	0	57	62,5	29,32	0,087	WSW	1	Cloudy.
	2	0	67,5	63,5	29,29		W	1	Cloudy.
6	7	0	55	62,5	29,64	0,155	NW	2	Cloudy.
	2	0	64,5	63,5	29,80		NNW	2	Cloudy.
7	7	0	55	62	30,10	0,014	N	2	Fine.
	2	0	67	63	30,16		NW	1	Fair.
8	7	0	55	62	30,20		SW	1	Fair.
	2	0	70	63,5	30,14		SW	1	Cloudy.
9	7	0	58	63	29,87	0,320	SW	2	Cloudy.
	2	0	67	64	29,78		SW	2	Fair.
10	7	0	55	63	29,87	0,090	SW	1	Cloudy.
	2	0	65	63	29,78		WSW	1	Cloudy.
11	7	0	55,5	62	29,55		W	1	Cloudy.
	2	0	64	63	79,55		NNW	1	Cloudy.
12	7	0	53	61,5	29,68	0,069	W	1	Cloudy.
	2	0	62	62,5	29,67		W	1	Cloudy.
13	7	0	56	61,5	29,60	0,065	W	1	Cloudy.
	2	0	65	62,5	29,46		SSW	2	Cloudy.
14	7	0	55	61,5	29,51	0,205	WSW	1	Cloudy.
	2	0	61	62	29,55		W	1	Cloudy.
15	7	0	55	61	29,75	0,050	W	1	Fine.
	2	0	68	61,5	29,84		W	1	Cloudy.
16	7	0	58,5	61	29,95		W	1	Fair.
	2	0	69	62,5	30,03		NW	1	Fair.

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1790	Time,		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
July 17	7	0	61	62	30,16		NW	1	Cloudy.
	2	0	70	63,5	30,16		NW	1	Cloudy.
18	7	0	61	63	30,08		W	1	Cloudy.
	2	0	71	64	30,04		SW	1	Cloudy.
19	7	0	59	64	30,06	0,060	W	1	Fine.
	2	0	72	65	30,04		W	1	Fine.
20	7	0	57	63,5	29,80	0,015	SW	2	Cloudy.
	2	0	64	64.5	29,63		SW	2	Cloudy.
21	7	0	57	63,5	29,77	0,026	W	2	Fair.
	2	0	65	64	29,86		WNW	2	Cloudy.
22	7	0	55	63	30,06		W	1	Cloudy.
	2	0	70,5	64	30,06		WNW	1	Fair.
23	7	0	59	63	30,00	0,015	WNW	1	Cloudy.
	2	0	59	63,5	29,99		W	1	Rain.
24	7	0	62	63,5	30,00	0,060	WNW	1	Cloudy.
	2	0	71	65	30,03		WNW	1	Cloudy.
25	7	0	58	64	30,13		WNW	1	Cloudy.
	2	0	71,5	65,5	30,14		W	1	Fine.
26	7	0	59	64	30,16		WSW	1	Cloudy.
	2	0	73,5	65,5	30,14		W	1	Fair.
27	7	0	59	64,5	30,15		W	1	Cloudy.
	2	0	73,5	66,5	30,15		WNW	1	Fair.
28	7	0	59,5	65	30,10		E	1	Cloudy.
	2	0	73	67	29,96		SSE	1	Fine.
29	7	0	58	65,5	29,60		NW	1	Cloudy.
	2	0	67	65,5	29,58		NW	1	Cloudy.
30	7	0	55	64	29,60	0,030	W	1	Cloudy.
	2	0	66,5	64	29,64		W	1	Cloudy.
31	7	0	56	63,5	29,46	0,303	WSW	2	Cloudy.
	2	0	65	64,5	29,61		WSW	2	Cloudy.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	53	63	29,93	0,012	W	1	Fine.
	2	0	66	64,5	30,00		W	1	Fair.
2	7	0	56	62,5	29,91		SW	1	Cloudy.
	2	0	59	62,5	29,81		SSW	1	Rain.
3	7	0	55	62	29,74	0,202	WSW	1	Cloudy.
	2	0	65	62,5	29,66		W	1	Rain.
4	7	0	54	61,5	29,83	0,081	W	1	Cloudy.
	2	0	67,5	62,5	29,86		W	1	Fair.
5	7	0	55	61,5	30,04		W	1	Cloudy.
	2	0	69	62	30,00		W	1	Cloudy.
6	7	0	60	62	29,91		W	1	Cloudy.
	2	0	66,5	63,5	29,86		WSW	1	Rain.
7	7	0	64	63,5	30,01	0,025	WSW	1	Cloudy.
	2	0	76	65	30,01		W	1	Cloudy.
8	7	0	62	65	29,88		W	1	Cloudy.
	2	0	71	66	29,94		W	1	Cloudy.
9	7	0	57,5	64,5	30,00		WNW	1	Fine.
	2	0	70	66	30,00		NW	1	Fine.
10	7	0	56,5	64,5	30,07		W	1	Fine.
	2	0	75,5	67	30,06		W	1	Fine.
11	7	0	56,5	65	30,14		W	1	Hazy.
	2	0	75	67	30,11		W	1	Hazy.
12	7	0	58	66	30,11		WSW	1	Fair.
	2	0	73	67	30,10		SW	1	Cloudy.
13	7	0	60,5	67	30,04		W	1	Fair.
	2	0	74	68	30,02		W	1	Cloudy.
14	7	0	60	67	30,10		WNW	1	Fine.
	2	0	72,5	68,5	30,10		NW	1	Fine.
15	7	0	59	67	30,02		SSE	1	Fine.
	2	0	75,5	69,5	29,95		SSW	1	Fine.
16	7	0	66	68	29,90		WSW	1	Cloudy.
	2	0	73	69	29,93		WSW	1	Cloudy.

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1790	Time. H.	M.	Therm. without °	Therm within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	60	68	29,95	0,020	W	1	Fair.
	2	0	68	68,5	30,01		WNW	1	Rain.
18	7	0	60	68	30,16	0,061	WNW	1	Cloudy.
	2	0	71	68	30,13		WNW	1	Fair.
19	7	0	59	67	30,02		W	1	Cloudy.
	2	0	72,5	68	30,00		W	1	Cloudy.
20	7	0	63	67,5	30,05		W	1	Cloudy.
	2	0	77	69,5	30,05		WSW	1	Fine.
21	7	0	65	68	29,78	0,060	SSE	1	Fine.
	2	0	76	73	29,73		W	1	Hazy.
22	7	0	56	68	30,04		W	1	Fine.
	2	0	67	68	30,08		W	1	Fine.
23	7	0	62	67	29,94		WSW	2	Cloudy.
	2	0	67	67,5	29,94		WSW	2	Cloudy.
24	7	0	53	65,5	30,01		W	1	Fine.
	2	0	68	66,5	29,96		W	2	Fair.
25	7	0	53	64	29,93		W	1	Cloudy.
	2	0	59,5	64,5	29,85		SE	1	Rain.
26	7	0	57	64,5	29,64	1,150			Rain.
	2	0	63	65	29,73		NW	1	Cloudy.
27	7	0	55	64	29,83	0,035	NW	1	Cloudy.
	2	0	58,5	64	29,80		N	1	Cloudy.
28	7	0	51,5	62	29,95	0,345	WNW	1	Cloudy.
	2	0	63	63,5	29,95		W	1	Cloudy.
29	7	0	53,5	62	30,02		W	1	Cloudy.
	2	0	59	62	30,04		W	1	Cloudy.
30	7	0	54	61,5	30,15		W	1	Cloudy.
	2	0	67	62,5	30,15		WSW	1	Fair.
31	7	0	59	62	30,08		WSW	2	Cloudy.
	2	0	73	64	30,06		W	2	Cloudy.

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1790	Time		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.		°	Inches.	Inch.	Points	Str.	
Sept. 1	7	0	62	63,5	29,91		W	1	Cloudy.
	2	0	68	64	29,88		W	1	Cloudy.
2	7	0	52,5	62,5	29,82		W	1	Fair.
	2	0	64	62,5	29,60		W	1	Cloudy.
3	7	0	49,5	61	29,31	0,210	W	2	Fine.
	2	0	58	61,5	29,43		W	2	Cloudy.
4	7	0	48	60	29,64		WNW	1	Fine.
	2	0	61	62	29,64		W	1	Cloudy.
5	7	0	45	59,5	29,67		W	1	Fine.
	2	0	55	61	29,77		WNW	1	Fair.
6	7	0	46	59	30,00		N	1	Fine.
	2	0	60	60	30,03		NNW	1	Fair.
7	7	0	46	58,5	30,11		NNW	1	Fine.
	2	0	55,5	60	30,05		N	1	Rain.
8	7	0	49	58,5	30,06	0,022	N by W	1	Fine.
	2	0	58	61,5	30,12		NNW	1	Fair.
9	7	0	49	58,5	30,19		W	1	Cloudy.
	2	0	62,5	59,5	30,12		W	1	Fair.
10	7	0	57	59	29,96	0,053	W	1	Cloudy.
	2	0	64	60	29,98		W	1	Fair.
11	7	0	54	59,5	29,98		W	1	Fine.
	2	0	64	61	30,04		W	1	Fair.
12	7	0	55,5	59,5	30,11		W	1	Cloudy.
	2	0	63,5	60,5	30,11		W	1	Cloudy.
13	7	0	56,5	60,5	30,00		W	1	Cloudy.
	2	0	67	62	29,94		W	1	Fair.
14	7	0	59	61,5	29,80		SSW	2	Cloudy.
	2	0	65	62,5	29,75		SW	2	Fair.
15	7	0	57	62	29,78		SSW	1	Cloudy.
	2	0	67	63,5	29,75		WSW	1	Fair.
16	7	0	49	61,5	29,98		WSW	1	Fair.
	2	0	62	62,5	30,10		W	1	Fine.

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1790	Time. H.	Time. M	Therm without °	Therm within °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Sept. 17	7	0	48	61	30,33		W	1	Fair.
	2	0	63	63	30,26		SW	1	Fine.
18	7	0	46,5	61	30,15		SW	1	Fine.
	2	0	68	64	30,08		SW	1	Fine.
19	7	0	56	62,5	29,85		SW	1	Cloudy.
	2	0	68	65	29,72		SW	2	Fair.
20	7	0	52	63,5	29,64	0,020	WSW	2	Fine.
	2	0	62	64	29,68		W	2	Fine.
21	7	0	50	60,5	29,83		W	2	Fine.
	2	0	59	61,5	29,99		WNW	2	Cloudy.
22	7	0	47	59	30,23		W	1	Fine.
	2	0	61,5	60,5	30,20		SSW	2	Cloudy.
23	7	0	58	60	29,90		SSW	2	Cloudy.
	2	0	66	61,5	29,90		W	2	Cloudy.
24	7	0	55	61,5	30,25	0,063	NW	2	Cloudy.
	2	0	61	61,5	30,34		NW	2	Cloudy.
25	7	0	53	60	30,38		NW	1	Cloudy.
	2	0	60	61	30,35		NW	1	Cloudy.
26	7	0	48	59,5	30,42		WNW	1	Cloudy.
	2	0	60	60	30,42		N	1	Cloudy.
27	7	0	52	59	30,36		NE	1	Fair.
	2	0	56	59	30,30		ESE	1	Cloudy.
28	7	0	42	58	30,14				Foggy.
	2	0	56,5	59	30,05		W	1	Fine.
29	7	0	50	58	30,17		N	1	Cloudy.
	2	0	59,5	59	30,22		N	1	Fair.
30	7	0	52,5	58	30,25		NE	1	Cloudy.
	2	0	59,5	59,5	30,20		E	1	Fair.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	46	58	30,03		E	1	Fine.
	2	0	57	59,5	29,95		E	1	Fine.
2	7	0	49	58	29,80		ENE	1	Cloudy.
	2	0	60	60	29,78		E	1	Fine.
3	7	0	56	59	29,77	0,083	E	1	Cloudy.
	2	0	64	60	29,82		S	1	Cloudy.
4	7	0	51,5	59	29,90		N	1	Cloudy.
	2	0	57	59,5	29,95		W	1	Cloudy.
5	7	0	52	59,5	30,00	0,401			Foggy.
	2	0	60	60,5	30,00		W	1	Fair.
6	7	0	54	60	29,80	0,132	SW	2	Cloudy.
	2	0	62	62	29,80		SW	2	Fair.
7	7	0	56	60,5	29,78	0,035	SSW	1	Cloudy.
	2	0	64,5	63	29,76		SW	1	Fine.
8	7	0	51	61,5	29,84		WNW	1	Fine.
	2	0	59	63,5	29,89		WNW	1	Fine.
9	7	0	52	61,5	29,89	0,101	N	1	Cloudy.
	2	0	54,5	63,5	29,94		NNW	1	Fine.
10	7	0	40	58	30,09		NE	1	Fine.
	2	0	50,5	59	30,08		N	1	Hazy.
11	7	0	39	56,5	30,11		N	1	Foggy.
	2	0	53,5	59	30,07		SSW	1	Fine.
12	7	0	51	57	29,86		WSW	1	Cloudy.
	2	0	58	57,5	29,66		SW	2	Cloudy.
13	7	0	53	56,5	29,71	0,073	SW	2	Fine.
	2	0	55,5	58	29,79		W	2	Fine.
14	7	0	52	56	29,80		SW	2	Cloudy.
	2	0	64,5	59,5	29,79		SW	2	Cloudy.
15	7	0	53,5	58,5	29,98		WSW	2	Fair.
	2	0	62	60,5	30,00		W	2	Cloudy.
16	7	0	[illegible]5	58	30,34		W	1	Cloudy.
	2	0	53,5	60	30,40		E	1	Fine.

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1790	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	46	57,5	30,20		E	1	Fine.
	2	0	57	60,5	30,01		SSE	1	Fine.
18	7	0	55	58	29,89		SW	2	Cloudy.
	2	0	55	60,5	29,86		W	1	Rain.
19	7	0	39	57,5	30,17	0,263	W	1	Fine.
	2	0	50	60	30,20		W	1	Fine.
20	7	0	41	57	30,05		E	1	Fine.
	2	0	55,5	59.5	29,96		E	1	Hazy.
21	7	0	52	58	29,93	0,020	E	1	Rain.
	2	0	60,5	62	29,87		E	1	Fine
22	7	0	54	60	29,75		E	1	Cloudy.
	2	0	62	62,5	29,70		E	1	Cloudy.
23	7	0	53	60,5	29,72		E	1	Cloudy.
	2	0	59.5	62,5	29,62		E	1	Cloudy.
24	7	0	51	60	29,72		SW	1	Cloudy.
	2	0	55,5	62,5	29,72		W	1	Cloudy.
25	7	0	50	60,5	29,70		WNW	1	Cloudy.
	2	0	56	6[illegible],5	29,77		WNW	1	Fair.
26	7	0	49	60	29,90		NE	1	Cloudy.
	2	0	56	62	29,90		NE	1	Cloudy.
27	7	0	46,5	60	29,85		NE	2	Cloudy.
	2	0	49	58,5	29,83		NE	2	Cloudy.
28	7	0	46	58	29,68		E	1	Cloudy.
	2	0	48	60	29,67		E	1	Cloudy.
29	7	0	46	58	29,77		E	2	Cloudy.
	2	0	48	60	29,83		E	2	Fair.
30	7	0	43	57	29,84		NE	1	Cloudy.
	2	0	45	57,5	29,83		NE	2	Cloudy.
31	7	0	40	55	29,86		NNE	1	Cloudy.
	2	0	45	58,5	29,90		N	1	Cloudy.

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1790	Time.		Therm. without	Therm within.	Barom	Rain.	Winds.		Weather.
	H	M.	°	°	Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	43	54	29,83		SE	2	Cloudy.
	2	0	50	56,5	29,66		S	2	Rain.
2	7	0	45	54,5	29,34	0,619	W	1	Cloudy.
	2	0	52	58	29,48		W	2	Cloudy.
3	7	0	41	54,5	29,80		W	1	Fine.
	2	0	50	58,5	29,78		W	1	Cloudy.
4	7	0	44	55	29,69	0,170	W	1	Fine.
	2	0	51	59	29,70		W	1	Fine.
5	7	0	46	56	29,95		W	1	Fair.
	2	0	52,5	58	29,94		S	1	Cloudy.
6	7	0	51	57	29,74		E	2	Cloudy.
	2	0	53	58	29,68		SSE	2	Rain.
7	7	0	44	56	29,64	0,057	E	1	Cloudy.
	2	0	47	57	29,66		E	1	Cloudy.
8	7	0	40	55	29,85		E	1	Fine.
	2	0	48	58	29,87		E	2	Fine.
9	7	0	43	56	30,04		NE	1	Cloudy.
	2	0	47	58	30,09		ENE	1	Cloudy.
10	7	0	43	55,5	30,20		ENE	1	Cloudy.
	2	0	46	57	30,06		ENE	1	Cloudy.
11	7	0	47	56	29,96		NE	1	Cloudy.
	2	0	48	57	30,03		NE	1	Cloudy.
12	7	0	47	56	30,11		NE	1	Cloudy.
	2	0	47,5	57,5	30,11		NE	1	Cloudy.
13	7	0	42	55,5	30,16		NE	1	Cloudy.
	2	0	52,5	58	30,19		NE	1	Fine.
14	7	0	43	55	30,27		NE	1	Cloudy.
	2	0	47	57	30,26		NE	1	Cloudy.
15	7	0	40	54,5	30,30		NE	2	Cloudy.
	2	0	44	56	3027		NE	2	Fine.
16	7	0	35,5	53	30,16		NE	2	Fine.
	2	0	43	55	30,11		ENE	2	Fine.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Nov. 17	7	0	40,5	53	29,98		ENE	1	Cloudy.
	2	0	43,5	55,5	29,91		ENE	2	Fair.
18	7	0	34	52,5	29,71		W	1	Cloudy.
	2	0	38	53,5	29,55		SW	1	Fair.
19	7	0	46	53	29,13		S	2	Rain.
	2	0	44	55	29,10		SSW	2	Cloudy.
20	7	0	43	53	29,16	0,295	S	2	Fine.
	2	0	48	56	29,21		S	2	Rain.
21	7	0	44	53	29,02	0,150	SW	2	Fair.
	2	0	48	57	29,10		SW	1	Rain.
22	7	0	45	54	29,27	0,950	S	1	Cloudy.
	2	0	48,5	58,5	29,38		N	1	Cloudy.
23	7	0	46	55	29,61	0,115	N	1	Foggy.
	2	0	48	58	29,68		SW	1	Cloudy.
24	7	0	35	54	29,83		SW	1	Cloudy.
	2	0	44,5	56	29,85		W	1	Fine.
25	7	0	47	54,5	29,73		SW	2	Cloudy.
	2	0	50	57	29,58		S	2	Cloudy.
26	7	0	49	55	29,48	0,156	WSW	1	Cloudy.
	2	0	48	57,5	29,74		ENE	1	Cloudy.
27	7	0	36	54	30,21		NW	1	Fine.
	2	0	42	57	30,28		NW	1	Cloudy.
28	7	0	37	53,5	30,40		N	1	Fair.
	2	0	43	58	30,34		E	1	Cloudy.
29	7	0	34,5	53	30,14		E	1	Cloudy.
	2	0	35	54,5	30,05		E	1	Cloudy,
30	7	0	32	50	29,72		E	1	Cloudy.
	2	0	34	51,5	29,66		E	1	Cloudy.

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1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	32	49	29,64		ESE	1	Cloudy.
	2	0	32	50,5	29,22		E	2	Snow.
2	8	0	43	51	29,31		WNW	2	Cloudy.
	2	0	46	54	29,59		WNW	2	Fair.
3	8	0	42,5	52	29,85		S	1	Rain.
	2	0	50	55	29,51		SSW	2	Cloudy.
4	8	0	42	53	29,94	0,220	W	1	Cloudy.
	2	0	44	56	29,97		W	1	Cloudy.
5	8	0	37	52	29,91		WNW	1	Cloudy.
	2	0	42	56	30,00		WNW	1	Cloudy.
6	8	0	36	52	30,36		NW	1	Cloudy.
	2	0	41	56,5	30,38		NW	1	Fine.
7	8	0	38	51,5	30,33		W	1	Cloudy.
	2	0	43,5	54,5	30,24		W	1	Cloudy.
8	8	0	47	54	30,15	0,105	NW	1	Cloudy.
	2	0	48	57	30,18		N	1	Cloudy.
9	8	0	47	54,5	30,18	0,023	NNW	1	Foggy.
	2	0	48,5	57,5	30,15		WNW	1	Cloudy.
10	8	0	47	56	30,19		W	1	Cloudy.
	2	0	50	58,5	30,19		W	1	Foggy.
11	8	0	49	56,5	29,96		W	2	Cloudy.
	2	0	52	58	29,85		SW	2	Rain.
12	8	0	47	54,5	30,06	0,015	W	1	Fine.
	2	0	43,5	58,5	30,08		W	1	Fine.
13	8	0	46,5	55,5	29,86	0,028	W	2	Cloudy.
	2	0	48	58	29,55		SSW	2	Cloudy.
14	8	0	39	55	29,85	0,444	W	2	Fine.
	2	0	43	57,5	30,00		WNW	2	Fine.
15	8	0	46	55	29,33	0,082	NW	2	Cloudy, much wind laſt night.
	2	0	48	57	29,40		W	2	Fair.
16	8	0	38	54	29,70		W	1	Cloudy.
	2	0	42	55,5	29,36		S	1	Rain.

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for December 1790.

1790	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	39	54,5	29,37	0,220	WSW	1	Fine.
	2	0	43	58	29,24		SW	1	Cloudy.
18	8	0	38	52	28,80	0,107	W	2	Cloudy, much wind laſt night
	2	0	39	53,5	29,15		W	2	Cloudy.
19	8	0	31,5	51,5	29,55		SW	1	Fine.
	2	0	33	53,5	29,15		SE	2	Snow.
20	8	0	33	50,5	29,90	0,185	SW	1	Fine.
	2	0	34,5	53	30,06		WNW	1	Fine.
21	8	0	43	50	29,90		SW	2	Cloudy.
	2	0	50	53	29,79		SSW	2	Rain.
22	8	0	38	51,5	30,06	0,070	W	2	Fine.
	2	0	43	54,5	30,17		W	2	Fine.
23	8	0	38	52	29,74	0,136	W	2	Fair, much wind, thunder and lightning laſt night.
	2	0	43,5	54	29,87		W	2	Fair.
24	8	0	37	52	30,24		W	1	Foggy.
	2	0	42	54,5	30,17		SW	1	Cloudy.
25	8	0	44	53,5	30,00	0,133	NW	1	Rain.
	2	0	44	54	30,08		N	1	Cloudy.
26	8	0	33	52	30,24		W	1	Foggy.
	2	0	35,5	53,5	30,22		SW	1	Cloudy.
27	8	0	30	49	30,04		WSW	1	Fair.
	2	0	38	53,5	29,90		SW	1	Cloudy.
28	8	0	33,5	50	29,97	0,020	WNW	1	Fine.
	2	0	36,5	54	30,07		NNW	1	Fine.
29	8	0	30	48,5	30,25		N	1	Fine.
	2	0	32	51,5	30,22		WSW	1	Fine.
30	8	0	37	48	29,94		S	2	Cloudy.
	2	0	39	50	29,84		SSW	2	Cloudy.
31	8	0	36	48	30,02	0,305	N	1	Cloudy.
	2	0	41	51,5	30,14		N	1	Cloudy.

	Therm without.			Therm. within.			Barometer.			Rain.
1790	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	
	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
January	52,5	32	40,9	56,5	50	53,1	30,47	29,27	30,07	0,967
February	55	33	43,8	59,5	52	54,9	30,62	29,88	30,25	0,115
March	56	33	45,4	59	52,5	56,8	30,65	29,83	30,26	0,122
April	58	35	44,0	59,5	51,5	55,5	30,30	29,38	29,86	1,470
May	66	47	56,3	66	58	60,9	30,14	29,50	29,90	2,898
June	86	53	60,5	73,5	60	63,1	30,35	29,49	30,03	0,708
July	73,5	53	62,2	67	61	63,5	30,20	29,29	29,84	1,700
Auguſt	77	51,5	63,4	73	61,5	65,2	30,16	29,64	29,97	1,991
September	68	45	56,6	65	58	60,8	30,42	29,31	30,00	0,368
October	64,5	39	52,4	63,5	55	59,5	30,40	29,62	29,89	1,108
November	53	32	44,3	59	50	55,4	30,40	29,02	29,81	2,512
December	52	30	40,9	58,5	48	53,6	30,38	28,80	29,88	2,093
Whole year			50,9			58,5			29,98	16,052

END OF PART I. OF VOL. LXXXI.

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OF THE

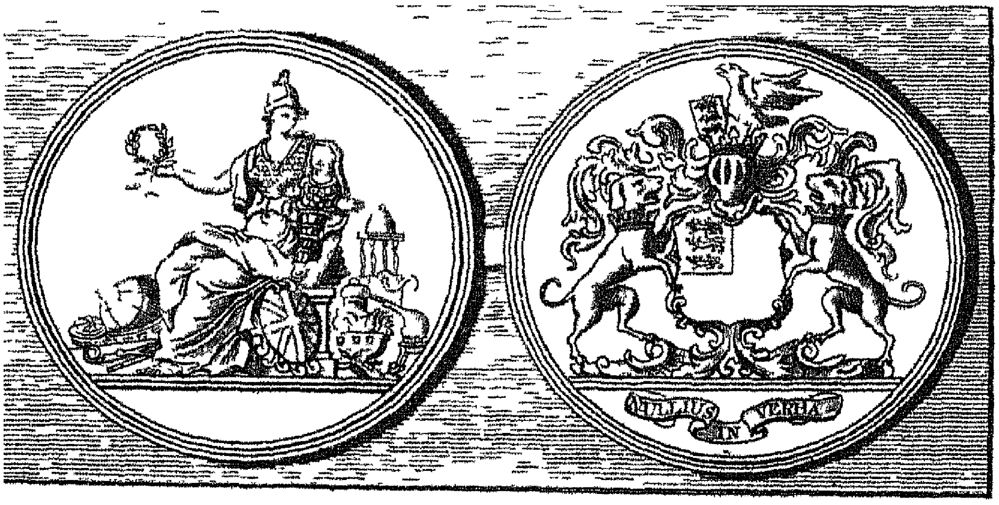
ROYAL SOCIETY

OF

LONDON.

VOL. LXXXI. For the Year 1791.

PART II.



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PHILOSOPHICAL TRANSACTIONS.

VIII. *On the Rate of Travelling, as performed by Camels; and its Application, as a Scale, to the Purposes of Geography.* By James Rennell, *Eſq. F. R. S.*

Read March 17, 1791.

AMONGST the diſcoveries and improvements of various kinds, that may be expected from the very laudable, and, as it concerns mankind in the groſs, no leſs humane and benevolent inſtitution of AN ASSOCIATION FOR PROMOTING DISCOVERIES IN THE INTERIOR PARTS OF AFRICA, that of the geography of the Continent in queſtion may be expected to make the quickeſt progreſs: ſince, in every kind of diſtant reſearch, whether ſucceſsful or otherwiſe, the act of enquiry alone brings materials to the geographer. But, even with every ordinary advantage, the geography of a continent muſt neceſſarily be ſlow in its progreſs towards maturity; *we*, therefore, can only expect to witneſs its early infancy; and cannot, from a view of the preſent, allow ourſelves to predict what its future features may be. Let it be our care, however, to foſter it; and contribute all within our power towards its improvement and perfection.

In a caſe where there is ſo little probability, even in a long courſe of time, of obtaining many fixed points by celeſtial obſervations (though I hope that one at leaſt will be attempted in the central part of Africa, in our time), it is fortunate that the mode of travelling happens to be ſuch, as ſerves to furniſh a remarkably equal ſcale: the rate of the camel's movement appearing to be, beyond all others, the leaſt variable; whether we examine it by portions of days, or of hours. In the preſent ſtate of things, the former mode alone can be uſed; becauſe few or none of the African travellers carry watches with them: but it may be hoped, that at no very diſtant period, the time employed on the road may be obtained with ſuch a degree of exactneſs, as to furniſh the geographer with materials of a far better kind, than any of thoſe, formed on computation, that have hitherto been exhibited.

Theſe remarks occurred on the reſult of an examination, which (though for a different purpoſe) I lately made into the rate of the camel's movement on the Arabian deſert, between Aleppo, Bagdad, and Buſſorah: for it appeared to me, that if the African caravans are compoſed of the ſame kind of camels, and are governed in their motions and œconomy by the ſame circumſtances, as thoſe which croſs the Arabian deſerts; there is no ſcale, of the computed kind, that can be more applicable to the African geography, than that formed on the camel's rate of travelling. I ſhall therefore detail the examples from whence I have drawn the proportions for the hours and days journey of the camel, under the two different degrees of burthen, which conſtitute what is commonly denominated the LIGHT, and the HEAVY caravan.

The routes which furniſh the above examples are determined in their horizontal, or direct diſtance, by the reſpective poſitions of ALEPPO, BAGDAD, and BUSSORAH: all of which have their latitudes and longitudes fixed by celeſtial obſervations *. Theſe routes are five in number: and although ſome of the journals that deſcribe them, contain leſs information than others, yet all of them have the time given with a ſufficient degree of preciſion, to enable me to found a general rule on. Three of theſe routes lead acroſs the GREAT DESERT, or that between Aleppo and Buſſorah; the other two are acroſs the LITTLE DESERT, or that between Aleppo and Bagdad.

The firſt of the Great Deſert routes was traced by a Mr. CARMICHAEL in 1751. The manuſcript copy of his Journal was obligingly communicated by my friend Dr. PATRICK RUSSELL; and it manifeſts a great degree of ingenuity and perſeverance in this way. The Author declares, that he was determined to keep a regiſter of the courſes by a compaſs, and to compute, comparatively, if not abſolutely, the intermediate diſtance on each courſe; by counting the ſteps or paces of the camel on which he rode, during a certain interval of time; and afterwards meaſuring a number of them on the ground. The particulars of this operation ſhall be given hereafter; and

---

* Aleppo, in Conn. des Temps, lat. 36° 11′, long. 37° 9′, reckoned from Greenwich.

Bagdad, by M. BEAUCHAMP, lat. 33° 22′, long. 44° 21′, Greenwich.

Buſſorah, by Capt. RITCHIE, lat. 30° 30′, long. 47° 33′, Greenwich.

although Mr. CARMICHAEL failed in the attempt to aſcertain his road diſtance by this method, yet his proceſs has furniſhed others with the means of aſcertaining the whole diſtance in the aggregate, and of proportioning the parts throughout. For, as the direct diſtance is given by the celeſtial obſervations, and a complete traverſe table by the journal, the *data* are perfect. And when the reader is informed that Mr. CARMICHAEL's whole line of bearing, *by compaſs*, between Aleppo and Buſſorah, nearly 720 Britiſh miles, coincided with the bearing line given by the celeſtial obſervations; by which it appears that the error could amount only to the mean quantity of the variation throughout, which might have been from ſix to ſeven degrees at that time (1751); he will give Mr. CARMICHAEL credit for much general accuracy. And it is not improbable, that even a conſiderable portion of the above error may have ariſen from the imperfection of his inſtrument *.

The ſecond journal was kept by Colonel CAPPER, in 1778, and was publiſhed ſeveral years ago; and the third, which contains little more than the time in detail, was communicated by my friend Mr. HUNTER, who croſſed the deſert in 1767.

The time given between Aleppo and Buſſorah, by theſe journals reſpectively, is as follows:

By Mr. CARMICHAEL	.	322 hours.
Colonel CAPPER	.	310
Mr. HUNTER	.	299½

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* I find, by Mr. DRUMMOND's chart of the road between Aleppo and Antioch (1747), that the variation was then about 6 degrees weſterly. This is proved by comparing his magnetic bearing line between thoſe places, with that given by the difference of latitude In the head of the Gulf of Perſia, the variation was 7 degrees in 1785.

But to ſhew that this difference aroſe chiefly from the variations in the route acroſs the CHALDEAN DESERT (between Mesjid Ali and Buſſorah; ſee the annexed ſketch, Tab. III.), I ſhall proceed, firſt, to explain that part of the ſubject; and afterwards to exhibit the particulars in proof.

Mesjid Ali (or Ali's Moſque) is ſituated at about two thirds of the diſtance, and as nearly as poſſible in the line of direction, between Aleppo and Buſſorah; and is a ſort of land-mark to the caravans which paſs the common boundary of the Arabian and Chaldean deſerts. Its geographical poſition is deduced from bearings and latitudes taken by M. NIEBUHR and others: and therefore, as far as general geography is concerned, it may almoſt be regarded as a fixed point. Not that the truth of its poſition will in any ſhape affect the preſent head of enquiry; which is entirely directed towards a compariſon of the ſpaces of time, employed between certain points of the route, by each traveller reſpectively.

Now, that portion of the Deſert route between Mesjid Ali and Buſſorah, being ſubject to great variation in the track, as appears by the journals of different travellers; whilſt the much larger portion of it, between Mesjid Ali and Aleppo, is very nearly the ſame at all times; it is very clear, that this latter portion furniſhes the propereſt ground on which to form the compariſon: and the particulars are as follow:

Aleppo

	CARMICHAEL.	CAPPER.	HUNTER.	
	H. M.	H. M.	H. M.	
Aleppo to Hagla .	11 5	11 24	10 0	Hagla.
Hagla to Ain il Koom .	37 30	41 4	35 0	to Taiba.
Ain il Koom to Uklet Hauran	80 10	78 41	81 30	to Uklet Hauran.
Hagla to Uklet Hauran .	117 40	119 45	116 30	
Uklet Hauran to Al Kadder	53 50	54 45	51 30	
Hagla to Al Kadder .	171 30	174 30	168 0	
Al Kadder to Rackama, oppoſite Mesjid Ali .	21 45	19 50	19 30	
Hagla to Rackama .	193 15	194 20	187 30	
Aleppo to Rackama .	204 20	205 44	197 30	

On the Little Deſert I have two examples of time, from Mr. IRWIN in 1781, and Mr. HOLFORD in 1780; both of whom kept regular journals.

	IRWIN.	HOLFORD.
	H. M.	H. M.
Aleppo to Ain il Koom.	52 0	46 27
Ain il Koom to Annah on the Euphrates . .	76 0	80 15
Aleppo to Annah . . .	128 0	126 42

It appears by the journals, that Mr. IRWIN deviated from the uſual track in the firſt part of his route; and that Mr. HOLFORD did the like in the latter part of his; each to avoid an enemy: ſo that it may be preſumed, that the deviations nearly balanced each other (ſee the ſketch).

Between Annah and Bagdad, theſe gentlemen made part of their journey in the caravan of loaded camels, and partly with *light* camels (that is, without any other load than the rider). Mr. IRWIN employed 62½ hours: but the laſt 15 hours, on light camels, were at an accelerated rate of half a mile *per* hour, or one fifth part, above the ordinary rate; according to his idea, which I have no doubt was a very juſt one: and this accelerated rate ſhould add 3 hours to the 15, to reduce it to caravan time; making 65½ hours inſtead of 62½. Mr. HOLFORD's journey, by the ſame *ratio*, muſt be reckoned at 68: but as this part of the two journies is obviouſly too inaccurate to draw any concluſions from, in the way of compariſon, I ſhall only make uſe of Mr. IRWIN's time (to which no ſolid objection can be urged) when I calculate the rate of the camel's travelling.

We have now ſeen, that on a journey of about 200 hours, between Aleppo and Mesjid Ali, two accounts differ only 1 hour 24 minutes; and a third differs from the mean of the other two ſeven hours and an half. And we may obſerve, that if the ſtage from Aleppo to Hagla be taken out of the queſtion, the number of Mr. HUNTER's hours would be nearer on an equality with the others by about an hour and a quarter (ſee p. 134.). The reaſon of the different reports of the diſtance between Aleppo and Hagla, appears to be, that travellers commonly join the caravans either at Hagla or on the road to it; and they, travelling by a quicker conveyance than camels afford, and then adjuſting the time to the caravan rate, make different eſtimates of the diſtance. Or there may be ſome other cauſe which has not been explained. Four different perſons give the time as follows:

	H.	M.		H.	M.
CARMICHAEL	11	5	CAPPER	11	24
HUNTER	10	0	HOLFORD	9	12

So that the proper point of outſet in making the compariſon, is Hagla. And, reckoning from thence, we have in the firſt table (p. 134.) the numbers 193¼, 194⅓, and 187½, for the time between Hagla and Meſjid Ali, in the three journies reſpectively: and the ſame table affords alſo the following comparisons between different places on the route:

In one inſtance, 80⅙, and 78⅔;
In a ſecond, . 117⅔, 119¾, and 116½;
In a third, . 53¾, 54¾, and 51½;
And in a fourth, 171½, 174½, and 168.

Again, between Aleppo and Annah on the Euphrates, the numbers in the ſecond table ſtand thus:

128, and 126⅔.

I think I need not produce any more examples to prove the equal rate of motion of a camel that is in any degree loaded; or rather of a number of camels together, where the rate will be determined by the *ſlow-going ones:* and whatſoever rate, in actual diſtance, may be deduced from theſe examples, muſt be applied to loaded camels travelling in a body together, and not to light camels, or thoſe choſen for ſpeed, whoſe rate appears to be at leaſt ⅕th greater. By a light camel is meant one that has only a man, or a very ſmall quantity of baggage, on it; whereas a camel's load is 500 to 600 pounds; and camels ſo loaded, form what is termed the HEAVY caravan. LIGHT caravan, on the contrary, is applied to camels under a *moderate* load, or perhaps little more than half loaded. And with reſpect to camels, either *moderately* or *fully loaded*, I can perceive no difference in their hourly rate of motion: the difference alone appears in the length of their day's journey; as we ſhall perceive hereafter. A camel, it is ſaid, will not permit himſelf to be over-laden; and this may be the reaſon why the load does not affect his rate of motion.

It appears, that the direct diſtance between Aleppo and Buſſorah, is 621 geographic miles, or 720 Britiſh, nearly. And Mr. CARMICHAEL's route, traced by a compaſs through all its principal bendings, and calculated trigonometrically, gives 688 geographic miles, or of Britiſh 797. It follows then, of courſe, that as the ſame gentleman was 322 hours on the road, the mean hourly rate of the camel's motion, was 2,475 Britiſh miles. Colonel CAPPER's route, though eaſily traced on the map, is not correct enough in its particulars, to ſerve as an authority equal to Mr. CARMICHAEL's; and the like may be ſaid of Mr. HUNTER's: but they muſt both be allowed to corroborate Mr. CARMICHAEL's in a general way; for as nearly as Colonel CAPPER's route can be traced, over the CHALDEAN Deſert (and, as we have before obſerved, the track is nearly the ſame at all times, in all other parts of the Deſert) the hourly rate of his camels was 2,51 *per* hour; and that of Mr. HUNTER's 2,585.

We come now to the LITTLE DESERT route. It has been noticed, that Mr. IRWIN employed 128 hours on his journey from Aleppo to Annah; and 65½ more (allowing for his accelerated rate 3 hours, ſee p. 135.) between Annah and Bagdad; altogether 193½ hours between Aleppo and Bagdad. The direct diſtance between thoſe places is 393 geographic miles; and by the route traced by Mr. IRWIN, the *road diſtance* comes out about 414½, or Britiſh miles 480 *. And this number, divided by 193½, gives 2,48 *per* hour for the camel's rate; or

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* Not that the diſtance between thoſe places is ſo much as 480 miles by the direct road: it is probably leſs than 470. But Mr. IRWIN's party took a circuitous courſe to the ſouthward, between Aleppo and Ain il Koom, to avoid an enemy that lay in the way. He eſtimated his diſtance at 493 miles.

within

within a very ſmall fraction of Mr. Carmichael's rate; his being, as we have juſt ſeen, 2,475.

I think I may venture to reſt the calculation of the loaded camel's mean hourly rate of travelling, on the experiments of Mr. Carmichael and of Mr. Irwin; both of whom appear to have taken much pains with the detail of their journies *; and then it will ſtand as under.

Mr. Carmichael on 322 hours	2,475	Mean 2,478 Britiſh miles.
Mr. Irwin on 193½ . .	2,48	

We have mentioned above, the reſult of Colonel Capper's and of Mr. Hunter's time, which gave a rate ſo very near to Carmichael's and Irwin's: and it may not be amiſs to add to theſe, the reſult of Mr. Holford's; as well as the eſtimates of the camel's rate, formed by ſeven different perſons. All theſe I have placed in one point of view, in the following table.

	Carmichael.	Irwin.	Capper.	Hunter.	Holford.	Plaisted.	Anonymous.
Eſtimated rates	Brit. mi. 2,29	2,55	2,25	2,33	2,24	2,3	2,5
Experiments	2,475	2,48	2,51	2,585	2,5	——	——

Mean of the ſeven eſtimates,	2,35.
Mean of the five experiments,	2,51.
Mean of Carmichael's and Irwin's,	2,478.

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* Mr. Irwin alſo took the bearings of his courſe by a compaſs, though not ſo much in detail as Mr. Carmichael; but Mr. Irwin not only remarked the time, but the particular rate of travelling, on each day; which appeared to vary from 2 to 3 *per* hour, but was commonly 2½; and the mean of all 2,55 Britiſh miles.

Before I quit the ſubject of the hourly rate, I ſhall obſerve, that the road diſtances in Mr. CARMICHAEL's traverſe table are often thrown together in lines of very conſiderable length, ſuch as 20 miles and upwards; and very commonly in lines of 5, 6, and 7, and yet are all conſidered as ſtraight lines. By this mode, it is probable, that many ſmall inflexions paſſed unnoticed: and therefore the rate taken from the reſults of CARMICHAEL's and IRWIN's obſervations, will be rather under the mark than otherwiſe; but it is obvious, that no kind of rule can be found to correct it by. It is certain, that fewer inflexions are likely to occur in the Deſert routes, than in almoſt any other; not to mention that the long lines of diſtance happen chiefly in the open, level part of the route. The road diſtance exceeds the direct diſtance, by *one-fourteenth part* of the latter only, between Aleppo and Mesjid Ali; amounting to a fraction of ,168 Britiſh mile on each hour; or in the proportion of 7¼ miles on each hundred of direct diſtance. This, I confeſs, is much leſs than I could poſſibly have ſuppoſed; and which nothing ſhort of actual experiment could have induced me to believe. On the whole road between Aleppo and Buſſorah, the proportion is nearly 11 on each hundred, on CARMICHAEL's route: but his route over the Chaldean Deſert was unuſually circuitous; and cannot be admitted to have any weight, in the determination of this queſtion.

One would expect that the inflexions of which no account is taken by Mr. CARMICHAEL, would amount to at leaſt half as much as thoſe which are taken notice of; and ſuch addition would make the whole hourly rate 2,56 inſtead of 2,475. But this is merely a ſuppoſition; and it is poſſible, that the rate may not be higher than 2,52, the mean of the four other experiments.

ments. At all events, the error can be but ſmall: and poſſibly, all circumſtances taken into the caſe (and particularly this remarkable one, that of three perſons who attempted to aſcertain the rate, by counting and meaſuring the camel's footſteps, none reckoned it higher than 2⅓, and one went ſo low as 2¼), I think the rate of two miles and an half *per* hour may be uſed, as differing but a ſhade from the general reſult; and as having the moſt manageable fraction.

Thus it appears to me, that the hourly rate of the camel may be applied as a very uſeful ſcale to the African geography; whenſoever the uſe of watches ſhall be adopted by the native travellers employed by the AFRICAN ASSOCIATION *; and with ſtill greater advantage, of courſe, if Europeans are employed. And if Mr. CARMICHAEL could deſcribe the general bearing, on a line of more than 700 Britiſh miles, ſo nearly as within 6 or 7 degrees of the truth; and that with a pocket compaſs; nothing more need be ſaid concerning the advantages that may be derived from the uſe of that valuable inſtrument, aided by ſuch a ſcale as I have been deſcribing.

The mean length of the day's journey of the camel, varies according to the degree in which it is loaded: and in this particular it is that the ſtate of the camel, as to its burthen, operates on its progreſs. It is neceſſary to obſerve, that whatſoever remarks I may offer, on the ſubject of the camel's day's journey, are meant to be applied only to the *mean* rate on journies of conſiderable length; ſince any other kind of experiment would be of no uſe in geography: I ſhall therefore conſider only the progreſs of the LIGHT and HEAVY CARAVANS, in which the camels are left to purſue their journey quietly and at leiſure; and with the regularity of a machine: and not that of the LIGHT CAMELS, which are not only freed from incumbrance, but are alſo urged on.

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* In the Memoirs of ABDUL KURRIM, a Caſhmerian of diſtinction, he informs us, that he kept an account of the time, on the road between *Bagdad* and *Mecca*, by means of an European watch, in the year 1740.

I have two examples of the heavy kind, and three of the light kind, where the time has been regularly kept: beſides a third example of the heavy kind, where the neceſſary regularity is wanting, but yet containing within itſelf, evidence ſufficiently ſtrong to corroborate the other two.

	H.	M.
The HEAVY caravans were thoſe of Mr. Carmichael and M. Holford; the firſt of 1000 camels, of which 600 were loaded, went, on a journey of 45 days, at a mean, each day, ...	7	10
The ſecond, with 50 loaded camels, on a journey of 15 days	7	40
Mean of the two,	7	25
The third, Teixeira, with 130 loaded camels, on a journey of 21 days, *about* . . .	7	30
Mean of all, *per* day,	7	27

The LIGHT caravans were,

			H.	M.
Meſſ. Irwin,	from 80 to 100 camels,	21 days,	9	12
Capper,		33 —	8	38
Hunter,		34 —	8	45
		Mean of the three	8	52

Here then the mean of the heavy caravan day is under ſeven hours and an half; and that of the light caravan between eight and three quarters, and nine hours.

Some of the ordinary watering places on the Deſert, being from 3 to 5 days journey aſunder, it may be ſuppoſed, that the length of the intermediate journies, would be determined by the known and approved ſtandard of a proper day's walk for a camel: for as they often arrive at the watering-place early in the day, it appears, that the caravan chiefs, do not, by any means, divide the ground between ſuch watering places, into equal portions for marches. This being the caſe, one may expect to arrive at the knowledge of what is at leaſt *intended* for a proper day's journey for a camel, under the circumſtances of the caſe: and indeed the reſult is ſuch as to prove what I have before advanced, reſpecting the length of the mean journies of both kinds. For having ſelected from the five above-mentioned journals, the length of the *apparently optional* journies in each; it appears, that the heavy caravans went 7 h. 51 m. on a mean of 24 ſuch days: and the light caravans 9 h. 8 m. on a mean of 38 days. In both of which caſes, as might have been expected, the length of the mean *optional* day, is ſomewhat longer than that of the whole journey; in which there is a wider field for delays and accidents. The compariſon is as follows:

Heavy caravan.				Light caravan.		
		Diſtance in Britiſh miles.			Diſtance in Britiſh miles.	
	H. M.	at 2½ *per* hour.	at 2,56	H. M.	at 2½ *per* hour.	at 2,56.
Mean daily rate of the whole journey . .	7 27	18,64	19,06	8 52	22,17.	22,7
Of optional days . .	7 51	19,62	20,1	9 8	22,8	23,38

Thus the mean daily rate of the heavy caravan, appears to be 18,64 Britiſh miles, reckoning two miles and an half for each hour; and 19,06 if taken at 2,56: and the mean rate of the light caravan 22,17 miles, at 2½; 22,7 at 2,56.

In order to apply this ſcale with effect, to the African geography, it is neceſſary to ſtate the number of days that the caravans uſually halt on the road; for as yet I have only conſidered their rate of motion: but it is evident, that if the length of the journey in the groſs, is given, the requiſite information will not be obtained, without a previous knowledge of the time loſt by neceſſary, or unavoidable halts on the road. My enquiries have furniſhed me with an account of 13 halts, to 149 days of travelling; or, which is the ſame thing, 13 halts out of 162 days, reckoned from the time of departure, to the time of the arrival of the caravans at the place of deſtination: that is, 1 halt to 12½ travelling days. This, of courſe, muſt be deducted from the aggregate of the diſtance: or, ſhould it be averaged on each day, the heavy caravan day muſt be reckoned at 17,14 miles inſtead of 18,64; and that of the light caravan 20,4, inſtead of 22,17; when the hourly rate is taken at two miles and an half.

It alſo remains to be ſtated, from the proportion that the road diſtance bore to the direct diſtance, by the trace of Mr. CARMICHAEL's route; what length in direct diſtance, and in geographic miles, may be allowed for each day, for the heavy caravan, on ſimilar lengths of journey, and over ſimilar tracts of country. It appears then, that on the 28 days between Aleppo and Rackama (oppoſite Mesjid Ali) the mean length of the day's journey, in direct diſtance, is about 15¼ geographic miles: and on the whole 45 days between Aleppo and Buſſorah, 13,8 ſuch miles. But this is without any allowance for halts; which, as has been obſerved before, require a deduction of 8 parts in 100, to be made from the groſs amount of the whole journey, when applied to the purpoſes of geography.

I have already taken notice, that Mr. CARMICHAEL counted the camel's ſteps, in order to aſcertain a ſcale of diſtance; and I ſhall now give the reſult of his obſervations, as well as of Mr. HOLFORD's, who alſo counted the ſteps, and meaſured the length of a number of them on the ground. Mr. HUNTER's experiment was on too ſmall a ſcale to ground a calculation on. It is certain, that theſe reports of the number of ſteps during certain portions of time, and the meaſurement of a certain number of thoſe ſteps on the ground, furniſh a reſult that does not agree with the experiments on the great ſcale; ſuch as we have juſt related, and which appear to be ſuſceptible of greater accuracy than thoſe made in detail. But it will, nevertheleſs, be proper to give the reſults, and to make ſome remarks on them; if be only to prevent any perſon in future from founding a calculation on them.

Mr. CARMICHAEL counted the double ſteps, or rather the return of the ſame foot, of a camel on which he rode, for an hour together, on 20 different days; at times when, from the nature of the ground, he thought the greateſt variation took place, in the rate of motion. He found the greateſt number of ſteps to be 2420, the leaſt 2086; and the mean of the whole 20 hours, was 2200. Mr. HOLFORD reckoned the greateſt 2240; leaſt 2060; mean 2150. They both report the double ſtep to be 5 feet and an half. The reſult of the former account is 2,29 Britiſh miles *per* hour; of the latter 2,24; and each allowed his diſtance accordingly, in his journal: though nothing is more certain than that their computed diſtances fall very ſhort of the truth. Mr. CARMICHAEL, for inſtance, reckons the diſtance from Aleppo to Buſſorah, by the road, at 720 Britiſh miles, although the direct diſtance itſelf ſcarcely falls ſhort of it. And Mr. HOLFORD's road diſtance alſo falls very ſhort. Even Mr. CARMICHAEL's higheſt number of paces, would exceed the mean rate given by the experiment at large, by a *ſixtieth* part only.

As theſe gentlemen's experiments differ only 50 ſteps, in the mean number, during the hour (one being, 2200, the other 2150), that is, a 44th part, the error muſt be looked for elſewhere; and it probably originated in their meaſuring too ſmall a number of ſteps on the ground to found their calculation on.

The reaſon of this great variation in the number of paces, in a given time, is the plenty or ſcarcity of the Deſert ſhrubs, on which the camels feed, as they go on; and thus ſuch expements become almoſt uſeleſs, unleſs the quality of the Deſert was deſcribed in every part. As the hourly rate of Mr. CARMICHAEL, coincides with that of Mr. IRWIN, within a very ſmall fraction, although the one travelled in November and December, the other in March and April; it appears, that the ſeaſons have little or no effect in this particular: and it is therefore highly probable, that the ſhrubs may flouriſh in ſome parts of the Deſert, and be dried up in others, at one and the ſame ſeaſon.

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IX. *On Infinite Series. By* Edward Waring, *M. D. F. R. S.* Lucaſian *Profeſſor of Mathematics in the Univerſity of* Cambridge.

Read March 24, 1791.

1. MERCATOR firſt publiſhed the contiuation of the common method of diviſion to an infinite ſeries of terms proceding according to the dimenſions of a variable quantity; NEWTON did the ſame for the common method of extraction of roots. Dr. BARROW before applied the ſame principles in ſome eaſy examples to find the aſymptotes of curves.

2. The methods of diviſion and extraction of roots were long before taught; but the continuation of them *in infinitum* would have been uſeleſs, as the areæ of curves, whoſe ordinates are $ax^{\frac{n}{m}}$ (where x denotes the abſciſs, and a, n, and m invariable quantities) had not been diſcovered many years before the time of MERCATOR's Publication, and conſequently it would have been of little uſe to transform an ordinate or fluxion, whoſe area or fluent is unknown, into another form, of which the area, &c. is equally unknown.

3. Sir ISAAC NEWTON extended the rule for raiſing a binomial (to any affirmative power) to negative powers, the extraction of roots and fractional indexes, by applying the law of the ſeries for affirmative powers to them, and continuing it *in infinitum*. M. DE MOIVRE extracted the root, &c. of a multinomial by a ſeries of a ſimilar nature; but theſe methods

will only apply in the moſt ſimple caſes, when not more than one root is to be extracted. In every complicate caſe (*viz.* the extraction of roots of quantities which involve the roots of compound quantities) of irrational quantities, recourſe muſt be had to the old methods of multiplication, diviſion, and extraction of roots.

4. If a root of a complicate irrational quantity be required by a ſeries proceeding according to the dimenſions of x; firſt reduce all the irrational quantities contained under the root by multiplication, diviſion, and extraction of roots into ſerieſes proceeding according to the dimenſions of x, ſo that the terms of the leaſt dimenſions be conſtituted firſt, if an aſcending ſeries be required, and ſo on; and the contrary, if a deſcending; then add the ſeveral ſums together, and extract the root of the reſulting ſum by a ſeries which proceeds according to the dimenſions of x, and it will be the root required.

Ex. Let the value of the quantity $\sqrt[3]{}\,(x^{-2}+a+\overline{b+cx}\times\sqrt[m]{}\,(d+ex+\sqrt{}(f+gx^2))+\sqrt{}(b+kx^2))$ be required by an aſcending ſeries; firſt, extract the innermoſt root $\sqrt{}(f+gx^2)=f^{\frac{1}{2}}+\frac{gx^2}{2f^{\frac{1}{2}}}-$ &c. which add to the quantity $d+ex$ contained under the ſame root (m), and place the terms of the ſum according to the dimenſions of x, and it becomes $(d+f^{\frac{1}{2}})+ex+\frac{gx^2}{2f^{\frac{1}{2}}}-$ &c. of which extract the (m) root, and there reſults

$$(d+f^{\frac{1}{2}})^{\frac{\;}{m}}+\frac{1}{m}\times\overline{d+f^{\frac{1}{2}}}^{\frac{\;}{m}}\times ex+\frac{1}{m}\times\frac{1-m}{2m}\times\overline{d+f^{\frac{1}{2}}}^{\frac{\;}{m}}\times e^2 \,\Big|\, x^2+\frac{1}{m}\times\overline{d+f^{\frac{1}{2}}}^{\frac{1-m}{m}}\times\frac{g}{2f^{\frac{1}{2}}}$$

&c. multiply this ſeries into $b+cx$, and thence is derived

$b\times$

$$\left.\begin{array}{l} b\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1}{m}}+\frac{1}{m}b\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1-m}{m}}\times e \\ \qquad + \quad c\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1}{m}} \end{array}\right\} x+\&c.=P;$$ extract the root

$\sqrt{\overline{h+kx^2}}=h^{\frac{1}{2}}+\frac{kx^2}{2h^{\frac{1}{2}}}+\&c.=Q$; add the three quantities P, Q, and $x^{-2}+a$ contained under the ſame root $\sqrt[3]{}$ together, and the ſeries reſulting, whoſe terms are conſtituted according to the dimenſions of x, will be $x^{-2}+(a+b\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1}{m}}+h^{\frac{1}{2}}=A)+(\frac{1}{m}b\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1-m}{m}}\times e+c\times\overline{\overline{d+f}^{\frac{1}{2}}}^{\frac{1}{m}}=B)x+\&c.$; of which extract the cube root $\sqrt[3]{}$, and it will be $x^{-\frac{2}{3}}+\frac{1}{3}Ax^{\frac{4}{3}}+\frac{1}{3}Bx^{\frac{7}{3}}+\&c.$ the root required.

5. The principal uſe of reducing quantities into ſeries proceeding according to the dimenſions of the variable quantity is as before mentioned for finding the area of a curve from its ordinate; or, which correſponds, the integral from its naſcent or evaneſcent increment; but the ſerieſes deduced ſhould converge, otherwiſe from them cannot be found the area or integral. In the Meditationes Analyticæ a method was firſt publiſhed of finding when theſe ſeries will converge and when not, *e. g.* the ſeries $a+bx+cx^2+dx^3+\&c.=\int(A+Bx+Cx^2+Dx^3+\&c.)^m\times\dot{x}=P$ will converge when (x) either affirmative or negative is leſs than the leaſt root (α) of the equation $A+Bx+Cx^2+Dx^3+\&c.=0$, if the roots are poſſible. A ſimilar rule is given when ſome of the roots are impoſſible. The ſeries will diverge when x is greater than α, and the caſes are given in which it will converge when $x=\alpha$. The ſeries deſcending according to the dimenſions of x will converge when x is greater than the greateſt root of the equation, &c. Theſe principles are further

ther applied in the ſame Book to complicate irrational algebraical functions of x, &c.

Hence moſt commonly the ſeries for the area contained between two ordinates, or integral between two different increments deduced by the common method will diverge; on which account, in the ſame Book, is given a method by interpolation of finding the area or integral contained between any two different values of x by converging ſeries, if the area, &c. is finite.

6. To find whether a given value $(+a)$ is leſs than the leaſt affirmative or negative root (x) of a given algebraical equation $A+Bx+Cx^2+Dx^3+\&c.=0$, if all its roots are poſſible; transform the equation into another, whoſe root z is the reciprocal of the root $x=\frac{1}{z}$ of the given equation, and for z in the reſulting equation write reſpectively $v+a$ and $v-a$; and if from the former ſubſtitution all the terms become negative or affirmative, and from the latter they become alternately negative and affirmative, then will a be leſs than the leaſt root of the given equation. If in the ſame manner, in the given equation for x be ſubſtituted $v+a$ and $v-a$, and the terms reſult as before, then will (a) be greater than the greateſt root affirmative or negative of the given equation.

7. When the integral of an algebraical quantity, whoſe increments are finite, is required; firſt, by the method given in Medit. Analyt. inveſtigate the integral in finite terms, if it can be expreſſed by them; but if not, reduce it into infinite ſerieſes of which the integral of each of the terms can be found, and alſo the ſerieſes for finding the integral contained between the two different given values of the variable quantity may converge.

Serieſes of this kind have been given in the Medit. Analyt. and innumerable of a like kind may be added for finding integrals by converging ſerieſes either aſcending or deſcending, of which the given increments are either finite or evaneſcent.

7. 2. It may be obſerved, that generally the particular caſe of which the increments are naſcent or evaneſcent may be deduced from the general, in which the increments are finite; and conſequently in many caſes the general will, *mutatis mutandis*, correſpond to the particular; *e. g.* 1. the integral cannot be expreſſed in finite algebraical terms, when any factor in the denominator of the increment has not a ſucceſſive correſpondent one; which is analogous to the caſe of the ſimple diviſor in the denominator of a fluxion publiſhed in the Quadrature of Curves. 2. Nor can it be expreſſed by the above-mentioned terms, when the dimenſions of the variable quantity in the denominator exceed its dimenſions in the numerator by unity, which correſponds to a ſimilar caſe in fluxions firſt given in Medit. Analyt. To theſe may ſeveral others be added.

8. Let the fluent of the fluxion $(A+Bx^n+Cx^{2n}+\&c.)^m \times x^{\theta-1}\dot{x} = ax^\theta + bx^{\theta+n} + cx^{\theta+2n} + \&c. = (A+Bx^n+Cx^{2n}+\&c.)^{m+1} \times \overline{\alpha x^\theta + \beta x^{\theta+n} + \gamma x^{\theta+2n} + \&c.} = a'x^\lambda + b'x^{\lambda-n} + c'x^{\lambda-2n} + \&c. = (A+Bx^n+Cx^{2n}+\&c.)^{m+1} \times \overline{\alpha' x^{\lambda'} + \beta' x^{\lambda'-n} + \gamma' x^{\lambda'-2n} + \&c.}$

1. If the infinite aſcending ſeries $\alpha x^\theta + \beta x^{\theta+n} + \gamma x^{\theta+2n} + \&c.$ converges, then will the ſeries $ax^\theta + bx^{\theta+n} + cx^{\theta+2n} + \&c.$ converge, and nearly in the ſame ratio; and *vice verſâ*, if the former diverges, the latter will alſo diverge. In the ſame manner if the infinite deſcending ſeries $\alpha' x^\lambda + \beta' x^{\lambda-n} + \gamma' x^{\lambda'-2n} + \&c.$ converges, then will the ſeries $a'x^\lambda + b'x^{\lambda-n} + c'x^{\lambda-2n} + \&c.$ converge; and if the former diverges, then will the latter alſo diverge; and in all the caſes nearly in the ſame ratio, except only when their convergency is the leaſt.

2. If the ſeries $ax^{\theta}+bx^{\theta+n}+cx^{\theta+2n}+\&c.$ converges, then will the ſeries $a'x^{\lambda}+b'x^{\lambda-n}+c'x^{\lambda-2n}+\&c.$ diverge, unleſs in caſes of the ſloweſt convergency, where $x=\pm v\sqrt[r]{\pm 1}$, and all the roots of the given equation are of the formula $\pm v\sqrt[r]{\pm 1}$; when $x=\pm v\sqrt[r]{\pm 1}$, the ſucceſſive terms of the infinite ſeries deduced from algebraical quantities by the preceding method will ultimately, that is, at an infinite diſtance come more near to a ratio of equality with each other than any aſſignable difference.

3. If the fluxion be $(A+Bx^{n}+Cx^{2n}\;.\;.\;.\;.\;x^{rn})^{m}\times x^{\theta-1}\dot{x}$, then will $\lambda=mrn+\theta$, and $\lambda'=\theta-rn$.

Many more propoſitions concerning infinite ſeries and their convergency are given in the Medit. Analyt.

4. Let the fluxion be $\overline{a+bx^{n}}^{m}\times x^{b}\dot{x}$, of which find the fluent in a ſeries aſcending according to the dimenſions of x^{n}, and it will be

$$a^{m}\times x^{b+1}\Big(\frac{1}{b+1}+\frac{m}{b+n+1}\times\frac{b}{a}x^{n}+m\cdot\frac{m-1}{2(b+2n+1)}\times\frac{b^{2}}{a^{2}}x^{2n}$$

$$+m\cdot\frac{m-1}{2}\cdot\frac{m-2}{3\;b+3n+1)}\times\frac{b^{3}}{a^{3}}x^{3n}+\&c.\Big)=\frac{b^{m}x^{mn+b+1}}{mn+b+1}+$$

$$\frac{mb^{m-1}ax^{mn+b+1-n}}{mn+b+1-n}+m\cdot\frac{m-1}{2}\times\frac{b^{m-2}a^{2}x^{mn+b+1-2n}}{mn+b+1-2n}+\&c.=\overline{a+bx^{n}}^{m+1}$$

$$\times\Big(\frac{x^{b+1}}{\overline{b+1}a}-\frac{\overline{m+1}(n)+b+1}{\overline{b+1}\cdot\overline{b+1+n}}\times\frac{b}{a^{2}}x^{b+n+1}+\frac{(\overline{m+1}(n)+b+1)\cdot(\overline{m+2}(n)+b+1)}{\overline{b+1}\cdot\overline{b+1+n}\cdot\overline{b+1+2n}}$$

$$\times\frac{b^{2}}{a^{3}}\times x^{b+2n+1}-\frac{(\overline{m+1}\cdot n+b+1)\cdot(\overline{m+2n}+b+1)\cdot(\overline{m+3n}+b+1)}{\overline{b+1}\cdot\overline{b+1+n}\cdot\overline{b+1+2n}\cdot\overline{b+1+3n}}\times$$

$$\frac{b^{3}}{a^{4}}x^{b+3n+1}+\&c.\Big)=(bx^{n}+a)^{m+1}\times\Big(\frac{1}{(mn+b+1)b}\times x^{b+1-n}-$$

$$\frac{\overline{b+1-n}a}{\overline{mn+b+1}\cdot\overline{mn+b+1-n}b^{2}}\times x^{b+1-2n}+$$

$$\frac{\overline{b+1-n}\times\overline{b+1-2n}\times a^{2}}{\overline{mn+b+1}\times\overline{mn+b+1-n}\times\overline{mn+b+1-2n}\times b^{3}}\times x^{b+1-2n}-\&c.\;(L).$$

The

firſt ſeries, which aſcends according to the dimenſions of x^n, terminates, when $\frac{mn+h+1}{n}$ is a whole negative number; the ſecond, which deſcends according to the dimenſions of the ſame quantity x^n, terminates when $\frac{h+1}{n}$ is a whole affirmative number. The fluent will terminate both ways when $\frac{h+1}{n}$ is a whole affirmative and m a negative number greater than $\frac{h+1}{n}$.

5. When m is a whole number or $=0$, the fluent $\int \overline{a+bx^n}^{m} x^b \dot{x}$ can always be found in finite terms of x, or in the abovementioned finite terms, together with the log. of x; which appears from reducing $\overline{a+bx^n}^{m}$ into ſimple terms $a^m + ma^{m-1} bx^n + m \cdot \frac{m-1}{2} a^{m-2} b^2 x^{2n} +$ &c, and multiplying them into $x^h \dot{x}$, and finding the fluents of the reſulting fluxions: but the ſeries found by the preceding method will not always terminate when m is a whole number, and the ſeries findable as above mentioned; when properly corrected, it may be rendered findable, or, which means the ſame thing, the ſeries may be made to terminate.

6. When the ſeries which expreſſes the fluent of a fluxion terminates, we may begin either from one end of the ſeries, or the other; for example, in finding the fluent of the fluxion $\overline{a+bx^n}^{m} \times x^h x$, either aſſume the ſeries $\overline{a+bx^n}^{m+1} \times (\alpha x^{b+1} + \beta x^{b+n+1} + \gamma x^{b+2n+1} +$ &c.) or the ſeries $\overline{a+bx^n}^{m+1} \times (A + Bx^n + Cx^{2n} +$ &c.); the former, as is before mentioned, terminates

when $\frac{mn+b+1}{n}$ is a whole negative number; the latter when $\frac{b+1}{n}$ is a whole affirmative number.

In like manner aſſume for a deſcending ſeries $(bx^n+a)^{m+1}\times|(\alpha' x^{b+1-n}+\beta' x^{b+1-2n}+\gamma' x^{b+1-3n}+\&c.)$, or $(bx^n+a)^{m+1}\times|(\mathrm{A}x^{\overline{-m+1n}}+\mathrm{B}x^{\overline{-m+2n}}+\mathrm{C}x^{\overline{-m+3n}}+\&c.)$; the former will terminate when $\frac{b+1}{n}$ is a whole affirmative; the latter when $\frac{mn+b+1}{n}$ is a whole negative number.

It appears, therefore, that a ſeries will terminate equally by an aſcending or deſcending ſeries; and the end of the one aſcending ſeries is the beginning of its correſpondent deſcending one. All theſe ſerieſes, which do not terminate, proceed on *in infinitum*; one term in the former ſeries becomes infinite, when $b+1+zn=0$; and in the latter (L) when $mn+b+1-zn=0$, z being a whole affirmative number.

10. It has been obſerved in the Medit. Analyt. that if ſome quantities contained in the given irrational ones are much leſs or greater than the reſt, it may be preferable in the former caſe to reduce them into ſerieſes not proceeding according to the dimenſions of x, but according to the dimenſions of thoſe quantities; and in the latter caſe according to the reciprocal dimenſions of them; and particularly ſo if the fluent or integral of the terms of the reſulting ſerieſes can be found in finite terms, or by tables already calculated.

From ſimilar principles to thoſe before given may be found when the reſulting ſeries will converge, and when not.

This method will in many problems be uſeful, when the value of a near approximate is known.

Of this caſe I ſhall ſubjoin a few examples, of which ſome have been already publiſhed in the Medit. Analyt.

Ex. 1. Let the ſine and coſine of a given arc A be reſpectively s and c; then will the ſine and coſine of an arc $A+e$, where e bears a very ſmall ratio to A, be reſpectively $s+\frac{ce}{r}-\frac{s}{1.2r^2}e^2-\frac{ce^3}{1.2.3r^3}+\frac{}{1.2\,3.4r^4}e^4+\&c.$ (the ſigns proceed by pairs alternately negative and affirmative) $= s\,(1-\frac{1}{1.2r^2}\times e^2+\frac{1}{1.2.3.4r^4}\times e^4-\&c.)+c(\frac{e}{r}-\frac{e^3}{1.2.3r^3}+\frac{}{1.2.3.4.5r}-\&c.)$; and $c-\frac{s}{r}e-\frac{1}{1.2r^2}ce^2+\frac{1}{1.2.3r^3}\times se^3+\&c.$ (the ſigns proceed as before by pairs alternately negative and affirmative) $=c(1\quad\frac{1}{1.2r^2}e^2+\frac{1}{1.2.3.4r^2}e^4-\&c.)-\frac{s}{r}(e\quad\frac{}{2.3r}e^3+\frac{}{2.3.4.5r^4}e^5-\&c.)$

This ſeries can alſo eaſily be derived from NEWTON's ſeries by plane trigonometry, and will converge much ſwifter than the ſeries $A\quad\frac{A^3}{1.2.3r^2}+\frac{A^5}{2.3.4.5r^4}-\&c.$ &c. if e bears a ſmall proportion to A.

Ex. 2. Let e be a ſmall quantity in proportion to t, and the given fluxion $\frac{\dot{t}}{r^2+(t+e)}=\frac{\dot{t}}{r^2+t^2}-\frac{2te+e^2}{(r^2+t\ )^2}\dot{t}+\frac{(2te+e^2)^2}{(r\ +t^2)^3}\times\dot{t}\quad\frac{(2te+e^2)^3}{(r^2+t^2)^4}\times\dot{t}+\&c.=\frac{\dot{t}}{r^2+t^2}\quad\frac{2t\dot{t}}{(r^2+t^2)^2}\times e\quad\frac{(\overline{r^2+t^2}-2^2\times t^2)\dot{t}}{(r^2+t^2)^3}\times e^2+2t\times\frac{(2\overline{r^2+t^2}-2^2t^2)\dot{t}}{(r^2+t^2)^4}\times e^3+\&c.$ (P); the co-efficient of a term $\dot{t}\times e^{2m}$ of this ſeries will be a fraction whoſe numerator is $\overline{r^2+t^2}^{m}-\overline{m+1}.\frac{m}{2}\times 2^2\times t^2\times\overline{r^2+t^2}\quad+\overline{m+2}.\frac{m+1}{2}\quad\frac{m}{3}.\ \ m.\quad\times 2^4\times t^4\times\overline{r^2+t^2}^{m-2}\quad\overline{m+3}\quad\frac{m+2}{2}.\frac{m+1}{3}.\frac{m}{4}\quad\frac{m-1}{5}.\frac{m-2}{6}$

×

$\times 2^6 \times t^6 \times \overline{r^2+t^2}^{m-3} + \&c.$, and denominator $(r^2+t^2)^{2m+1}$; and the co-efficient of a term $\dot t \times e^{2m+1}$ will be a fraction whoſe numerator is $2t \times (\overline{m+1} \cdot \overline{r^2+t^2}^{m} - \overline{m+2} \cdot \frac{m+1}{2} \cdot \frac{m}{3} \times 2^2 \times t^2 \times \overline{r^2+t^2}^{m-1} + \overline{m+3} \cdot \frac{m+2}{2} \cdot \frac{m+1}{3} \cdot \frac{m}{4} \cdot \frac{m-1}{5} \times 2^4 t^4 \times \overline{r^2+t^2}^{m-2} -$ &c.) and denominator $(r^2+t^2)^{2m+2}$; m being a whole number. The continuation of theſe ſerieſes is too evident to need enunciation, as muſt generally be the caſe when the number of factors contained in the ſucceſſive terms continues the ſame or increaſes in arithmetical progreſſion; and the factors themſelves increaſe or diminiſh in an arithmetical progreſſion: the terms proceed alternately + and − by pairs.

2. 2. Let the given fluxion be $\frac{\dot e}{r^2+(t+c)^2} = \frac{\dot e}{r^2+t^2} - \frac{\overline{2te+e^2} \times \dot e}{(r^2+t^2)^2} + \frac{(2te+e^2)^2 \times \dot e}{(r^2+t^2)^3} - \&c. = \frac{\dot e}{r^2+t^2} - \frac{2t\dot e}{(r^2+t^2)^2} \times e - \frac{(r^2+t^2-2^2t^2)\dot e}{(r^2+t^2)^3} \times e^2 + \&c.$ (Q); this ſeries becomes the ſame as the preceding by ſubſtituting in it $\dot e$ for $\dot t$; in this ſeries e is conſidered as variable, in the preceding t.

The fluent of the former ſeries (P) is $\int \frac{\dot t}{r^2+t^2} + \frac{e}{r^2+t^2} - \frac{t}{(r^2+t^2)^2} \times e^2 - \frac{(r^2+t^2-2^2t^2)}{3(r^2+t^2)^3} \times e^3 - \&c.$; the fluent of the latter ſeries (Q) is the ſame as of the former (P) except the firſt term $\int \frac{\dot t}{r^2+t^2}$: the co-efficient of the term e^b in the fluents will be the ſame as the co-efficient of the term e^{b-1} in the ſerieſes (P) and (Q) divided by b.

This ſeries, when the tangent of a given arc is known, finds the arc whoſe tangent differs very little from the given tangent.

11. Let t be increaſed or diminiſhed by a quantity e in (P), where e bears a very ſmall ratio to any root or value of t in

the equations $P = 0$ or $\frac{1}{P} = 0$; and the reſulting quantity expanded into a ſeries $a + be + ce^2 + de^3 +$ &c. proceeding according to the dimenſions of e, and this ſeries be multiplied reſpectively into $\dot{t}$ and $\dot{e}$; and the fluents of the reſulting fluxions found; then will the former differ from the latter by $\int a\dot{t}$; for the former will be $\int a\dot{t} + ae + \frac{\dot{a}}{2t} \times e^2 + \frac{\dot{a}}{2 \cdot 3t^2} \times e^3 + \frac{\dot{a}e^4}{2 \cdot 3 \cdot 4t^3} +$ &c., and the latter $ae + \frac{\dot{a}}{2t} e^2 + \frac{\dot{a}e^3}{2 \cdot 3t^2} +$ &c.

2. In the ſame manner, if more than one variable quantities x, y, z, &c. are contained in (P), which are increaſed by ſmall increments or decrements α, β, γ, &c., may the increments or decrements of the quantity (P) be deduced from the incremential theorem.

Ex. Let the quantity (P) be $(a + bx^n)^m$, and a, b, x, n, and m be increaſed by very ſmall increments α, β, γ, δ, and ε; then will $(a + \alpha + \overline{b + \beta} \times \overline{x + \gamma}^{n+\delta})^{m+\varepsilon} = (a + bx^n)^m + (a + bx^n)^m \times \log. \overline{a + bx^n} \times \varepsilon + m \times (a + bx^n)^{m-1} \times (\alpha + \beta x^n + b\,(x^n\delta \times \log. x + nx^{r-1}\gamma)) +$ &c.

The terms are to be ſo placed, according to the dimenſions of the increments or decrements, that the greateſt may firſt occur.

12. Let ſome compound quantities be increaſed by any ſmall quantities variable or invariable, but not the variable quantities contained in them; then reduce the given quantity into a ſeries proceeding according to the dimenſions of the ſmall quantities, and find the fluent, integral, &c. as required.

Ex. Let the given quantity be $\frac{x^m\dot{x}}{(x^n + a^n + \beta)^r}$ $\frac{x\ \dot{x}}{(x^n + a^n)^r}$

$r\beta$

$\frac{r\beta\dot{x}x^m}{(x^r+a^n)^{r+1}} + r\cdot\frac{r+1}{2}\times\frac{\beta^2\dot{x}x^m}{(x^n+a^n)^{r+2}} - r\cdot\frac{r+1}{2}\cdot\frac{r+2}{3}\times\frac{\beta^3\dot{x}x^m}{(x^n+a^n)^{r+3}} +$ &c., where the quantity x^n+a^n is increaſed by a ſmall quantity β; then will $\int\frac{x^m\dot{x}}{(x^n+a^n+\beta)^r} = \int\frac{x^m\dot{x}}{(x^n+a^n)^r} - \frac{\beta}{n\times a^n}\left(\frac{x^{n+1}}{(x^n+a^n)^r} - \overline{m+1-rn}\times P\right) + \frac{r\beta^2}{2\cdot n\times a^n}\left(\frac{x^{m+1}}{(x^n+a^n)^{r+1}} - (m+1-\overline{r+1}n)\times Q\right)$ $\frac{r\cdot\overline{r+1}\beta^3}{2\cdot3\cdot n\times a^n}\left(\frac{x^{m+1}}{(x^r+a^n)^{r+2}} - (m+1-\overline{r+2}n)\times R\right) + \frac{r\cdot\overline{r+1}\cdot\overline{r+2}\cdot\beta}{2\cdot3\cdot4\cdot n\times a^n}$ $\left(\frac{x^{m+1}}{(x^n+a^n)^{r+3}} - (m+1-\overline{r+3}n)\times S\right)$ &c. The continuation of the ſeries is evident; the letters P, Q, R, S, &c. denote the fluents $\int\frac{x^m\dot{x}}{(x^n+a^n)^r}$, $\int\frac{x^m\dot{x}}{(x^n+a^n)^{r+1}}$, $\int\frac{x^m\dot{x}}{(x^n+a^n)^{r+2}}$, &c.

From the length of the arc of an hyperbola or ellipſe given may be deduced by this ſeries the length of a correſpondent arc of an hyperbola or ellipſe, of which the equations expreſſing the relation between its abſciſſæ and ordinates differ only by very ſmall quantities from the equations expreſſing the relation between the abſciſſæ and ordinates of the former.

13. The ſame principles may be applied to the reſolution of algebraical, fluxional, incremential, &c. equations.

Ex. Let L*m*M, &c. (Tab. IV. fig. 1.) be a given circle, whoſe center is C and radius (r), and it be required to find an arc LM, ſo that the area LSM deſcribed round a given point S contained between the lines LS, SM, and the arc of the circle LM be equal to a given area (α).

Find an arc Lm = A nearly equal to the arc required LM, of which to radius 1 ſubſtitute s for the ſine, and c for the coſine, and write e for mM = LM − Lm and b for SC; then will LM $\times\frac{r}{2}(\overline{A+e}\times\frac{r}{2}) \pm$ SC $\times\frac{r}{2}\times$ ſin. s arc : LM$\left(\frac{b}{2}\times(A+e-\right.$

A + e

$\frac{\overline{A+e}^3}{2\cdot 3r^2}+\frac{\overline{A+e}^5}{2\cdot 3\cdot 4\cdot 5r^4}$ &c.)) $=\frac{A}{2}\times r\pm\frac{b}{2}\times rs+\frac{r}{2}\quad\frac{\ }{2}\times c\times e\mp\frac{b}{2r}\times\left(\frac{1}{2}\times se^2+\frac{1}{2\cdot 3r}\times ce^3-\frac{1}{2\cdot 3\cdot 4r^2}\times se^4-\frac{\ }{2\cdot 3\cdot 4\cdot 5r^3}\times ce^5+\right.$ &c.$\left.\right)=\alpha$; in this equation for $\frac{2\alpha-Ar\mp b\times rs}{r\pm br}$ ſubſtitute π, and for $r\pm bc$ write t, and the equation reſulting will be $e\mp\frac{\ }{2tr}se^2\quad\frac{\ }{1\cdot 2\cdot 3tr}\times ce^3\pm\frac{b}{1\cdot 2\cdot 3\cdot 4tr^3}\times se^4\pm\frac{\ }{1\cdot 2\cdot 3\cdot 4\cdot 5tr}\times ce^5\mp$ &c. $=\pi$. From it find e in terms proceeding according to the dimenſions of π, and there reſults $e=\pi\pm\frac{bs}{2tr}\times\pi^2\pm\frac{3b^2s^2\pm bct}{2\cdot 3t^2r^2}\times\pi^3\pm$&c.

This method of reſolving KEPLER's and other problems of cutting a given area deſcribed round a point, whether focus or not, in a circle, when an approximate ſufficiently near to the area to be found is given, will converge as ſwift as any known method.

The reſolution of this problem may be deduced ſomewhat different by the following methods. Let the letters b, r, α, denote the ſame quantities as before, and s be the ſine of the arc Lm to radius 1, and $s+o$ the ſine of the arc LM nearly $=$ Lm; then will $\frac{r^2}{2}\int\frac{\dot{s}}{\sqrt{(1-\overline{s+o}^2)}}\pm\frac{br}{2}\times\overline{s+o}=\alpha$; reduce the fluxion $\frac{\dot{s}}{\sqrt{(1-\overline{s+o})}}$ into a ſeries $P\dot{s}+Qo\dot{s}+Ro^2\dot{s}+So^3\dot{s}+$ &c. and find the fluents of the fluxions $Q\dot{s}$, $R\dot{s}$, $S\dot{s}$, &c., which let be B, C, D, &c., there will reſult the equation $o+\frac{C}{t}o^2+\frac{D}{t}o^3+$&c. $=\frac{2\alpha\mp brs-r\times A}{t}=\pi$, where $t=Br^2\pm br$ and $A=$arc Lm; find o in terms of a ſeries proceeding to the dimenſions of π, and conſequently $s+o$ the ſine of the arc LM required.

From ſimilar principles may be found the tangent of the arc required from the equation $\frac{r^2}{2}\int\frac{\dot{t}}{1+(t+i)^2} \pm \frac{\overline{t+i}\times r\times b}{2\sqrt{(1+\overline{t+i}^2)}} = \alpha$, where the tangent t to rad. 1 of the arc Lm = A is given. Let the fluent of the fluxion $\left(\frac{\dot{t}}{1+(t+i)^2}\right) = A + Bi + Ci^2 + Di^3 + \&c.$; and $\frac{\overline{t+i}\times b}{\sqrt{(1+\overline{t+i}^2)}} = A' + B'i + C'i^2 + D'i^3 + \&c.$, the equation will become $i + \frac{rC\pm C'}{rB\pm B'}i^2 + \frac{rD\pm D'}{rB\pm b'}\times i^3 + \&c. = \frac{2\alpha - r^2A \mp rA'}{(rB\pm rB)} = \pi$; from this equation inveſtigate $i = \pi + P\pi^2 + Q\pi^3 + \&c.$, and thence $t+i$ the tangent required.

In like manner may be found the ſecant, coſine, &c. of the arc required.

14. The ſame principles may be applied to cut an area deſcribed round any given point in a given curve equal to an area α.

Let x be the abſciſs and y the ordinate of the given curve, and b the diſtance of the beginning of the abſciſs from the given points, and let (A) be the area of the curve deſcribed round the point S, when the abſciſs is x, which differs very little from the given area (α); to find the value $x+e$ of the abſciſs, when the area $=\alpha$.

Let $y = X$ a function of x, and in X for x write $x+e$, and reduce the reſulting quantity (X′) into a ſeries $X + Be + Ce^2 + De^3 + \&c.$ proceeding according to the dimenſions of e; then will the area $\int y\dot{x} = \int X\dot{x} + e\int B\dot{x} + e^2\int C\dot{x} + e^3\int D\dot{x} + \&c. = A + he + ke^2 + le^3 + \&c.$, and conſequently $A + he + ke^2 + le^3 + \&c. \pm (b \pm \overline{x+e}) \times \frac{y}{2} = A + he + ke^2 + le^3 + \&c + \frac{1}{2}(b \pm \overline{x+e}) \times (X + Be + Ce^2 + De^3 + \&c.) = A + \frac{1}{2}(b \pm x) \times X + (b + \frac{1}{2}\overline{b \pm x} \times$

$B + \frac{1}{2}X)e + (k + \frac{1}{2}(b \pm x)C + \frac{1}{2}B)e^2 + (l + \frac{1}{2}\overline{b \pm x}^2 \times D + \frac{1}{2}C)e^3 +$ &c. $= \alpha$; find the value of e in a ſeries proceeding according to the dimenſions of $\frac{\alpha - A - \frac{1}{2}(b \pm x) \times X}{b + \frac{1}{2}\overline{b \pm x} \times B + \frac{1}{2}X}$, and $x + e$ will be the abſciſs required; X denotes the value of y, when the abſciſs $= x$.

From the ſame principles may the ordinate y, &c. be found.

This problem may be reſolved in the ſame manner, when X denotes an infinite ſeries deduced from an equation expreſſing the relation between the abſciſs and ordinate of the given curve.

If the given area α be the difference between two areas SPM (α') and SPQ $= \beta$ (fig. 2.); for α ſubſtitute $\alpha' - \beta$, and the operation will be the ſame as the preceding.

15. 1. Given any equations, of which the increments of the quantities contained in them can be found from each other, and given approximate values of each of the unknown quantities, which nearly correſpond to each other; to find approximations, which differ leſs from the quantities themſelves than the given ones.

Suppoſe each of the given approximate values to be increaſed or diminiſhed by ſmall increments or decrements, as e, o, i, &c. which are the approximations to be found; and from the given find the equations reſulting from this hypotheſis; and from theſe may be deduced, by ſimple equations, the approximations ſought e, i, o, &c. by neglecting in them all the powers of e, o, i, &c. except the ſimple ones, and all the products of them multiplied into each other; and conſequently the equations deduced will contain only given quantities and ſimple powers of the unknown e, i, o, &c. to be found.

2. When two or more (n) values of one (x) of the unknown quantities are nearly equal to its given approximate; then the

equation which finds the approximate to x will be a quadratic or an equation of n dimenſions.

3. The approximations found by this method will converge more or leſs, according as the approximations given are more or leſs near to one value of each of the unknown quantities than to the remaining ones, &c.

Theſe principles were printed in the Medit. Algebr. in the years 1768 and 1769.

LEMMA.

15. Let *kab* (fig. 3.) be a circle, whoſe center is o; and Po perpendicular to the plane of the circle; and the force of any corpuſcle in the circle on the particle P vary as the corpuſcle divided by the (n) power of its diſtance from the particle; to find the attraction of the circle *kab* on the particle P.

From the ſuppoſita the force of any ring contained between the neareſt concentric circles *lcd* and *hef* of which the center is o, on the particle P, will be as the area of the ring divided by the nth power of the diſtance Ph; and conſequently if $\mathrm{P}o = \mathrm{A}$, $oh = v$, $hl = \dot{v}$, and $p =$ periphery of a circle of which the radius is 1; the attraction of the ring on P will be as $\dfrac{p \times v\dot{v}}{(\mathrm{A}^2+v^2)^{\frac{n}{2}}}$, and the force of it in the direction Po as $\dfrac{\mathrm{P}o}{\mathrm{P}h} \times \dfrac{pv\dot{v}}{(\mathrm{A}^2+v^2)^{\frac{n}{2}}} = \dfrac{p\mathrm{A}v\dot{v}}{(\mathrm{A}^2+v^2)^{\frac{n+1}{2}}}$; of which fluxion the fluent is $\dfrac{p\mathrm{A}}{\overline{1-n} \times (\mathrm{A}^2+v^2)^{\frac{n-1}{2}}}$; and thence the force of the circle *kab* on the particle P will be as $\dfrac{p \times \mathrm{A}}{\overline{1-n}\mathrm{P}k^{n-1}} - \dfrac{p \times \mathrm{A}}{\overline{1-n}\mathrm{PO}^{n-1}} \left(\dfrac{p}{\overline{1-n}\mathrm{A}^{n-2}}\right)$.

Cor. 1. The force of the area contained between the two circles *kab* and *lcd* on the particle P will be as $\frac{pA}{1-nPk^{n-1}} - \frac{pA}{1-nPl^{n-1}}$.

15. Let a ſolid be generated by the rotation of a given curve round a line in the axis P*o* or P*o* produced, and A the diſtance of P from *o*, and x = diſtance of any circle generated by the rotation of a point in the curve round the axis P*o* from *o*; and y the ordinate to the given curve, of which the abſciſs is x; and the ſame things be ſuppoſed as before; then will the attraction of the ſolid on the point P vary as

$$\int\frac{p\times\overline{A-x}\times\dot{x}}{\overline{n-1}\,(A-x)^{n-1}}\left(\frac{p\dot{x}}{\overline{n-1}\,(A-x)^{n-2}}\right)-\int\frac{p\times\overline{A-x}\times\dot{x}}{\overline{n-1}\times(A-x\ +y^{2})^{\frac{n-1}{2}}}$$

Let the given curve be a circle, of which the center is o, and radius t, and conſequently $y^2=t^2-x^2$, and $\int\frac{p\dot{x}}{\overline{n-1}\,(A-x)^{n}}$

$$-\int\frac{p\times A-x\times\dot{x}}{\overline{n-1}\,.\,(A-x\ +y^{2})^{\frac{n}{2}}}\qquad \overline{n-1}\,.\,n-3\times(A-x)^{n-3}$$

$$\int\frac{p\dot{x}\times\overline{A-x}}{\overline{n-1}\times(A^2+t^2-2Ax)^{\frac{n}{2}}}\qquad \overline{n-1}\,.\,n-3(A-x)^{n-3}\qquad \frac{p}{n-1}\qquad +$$

$$\frac{A^2+t^2}{n-5\,.\,n-3A^2}\qquad \frac{x}{n-5A}=\frac{\overline{n-4}A^2+t^2-\overline{n-3}Ax}{n-5\,.\,n-3\times A^2}\Big)\times\left(A^2+t^2-2Ax\right)^{\frac{3-n}{2}}.$$

By ſubſtituting $-t$ and t for x in the preceding expreſſion the attraction of the whole globe on the particle P will be found $\frac{p}{n-1}\left(\frac{A^2+\overline{n-3}At+t^2}{n-5\,.\,n-3\times A^2}\times \overline{A+t}^{\,3-n}\,(L) - \frac{A^2-\overline{n-3}At+t^2}{n-5\,.\,n-3\times A^2}\times\right.$ $\left.\overline{A-t}^{\,3-n}\,(H)\right)$.

16. If the particle be ſituated within the globe, and conſequently A leſs than t or $-t$, and n be an odd number negative

or affirmative; or fraction, of which the numerator is odd; then the preceding reſolution will be phyſically juſt; for in this caſe, if the attraction on one ſide of the particle be affirmative, the attraction on the other will mathematically be negative, that is, phyſically oppoſite: but if n be an even number, or fraction, of which the numerator is an even and denominator an odd number, the mathematical ſolution will not agree to the phyſical; for in the former the force on both ſides will be affirmative, in the latter the forces will be oppoſite, and therefore phyſically the force in this caſe will vary as $L+H$, and not as $L-H$, which is the force in both the caſes when A is greater than t and $-t$. The ſame may be applied to the more general reſolution.

17. Let ABCD (fig. 4.) be a globe, of which the diameters AB and CD are ſituated at right angles to each other, and AHBL be a ſpheroid generated by the revolution of an ellipſe on its axis AB, to which let HL nearly equal to $CD=AB$ be the conjugate, and P a point in the axis BA produced; to find the attraction of the ring contained between the globe and the ſpheroid on the point P, on the ſuppoſition that the force of any corpuſcle in the ring on the particle P varies as the magnitude of the corpuſcle directly, and the nth power of its diſtance from the corpuſcle inverſely.

Let $AB=CD=2t$, $CD=HL\ (2c)+2CH=2c+2e$, and conſequently $OH=c=t-e$, where e has a very ſmall ratio to t, $oP=A$, $po=x$, and pM parallel to $CD=y$; then, by the preceding lemma, the attraction of the circle whoſe radius is pM on the point P will be $\frac{p\times\overline{A-x}}{\overline{n-1}\,Pp^{n-1}}-\frac{p\times\overline{A-x}}{\overline{n-1}\,PM^{n-1}}$; and in like manner the attraction of the circle, whoſe radius is pm, will be $\frac{p\times\overline{A-x}}{\overline{n-1}Pp^{n-1}}-\frac{p\times\overline{A-x}}{\overline{n-1}\,.\,Pm^{n-1}}$, and conſequently the attraction of the ring contained between the two circles is $\frac{p\times\overline{A-x}}{\overline{n-1}\,.\,Pm^{n-1}}$

$$-\frac{p\times\overline{A-x}}{\overline{n-1}\,.\,PM^{n-1}}=\frac{p}{n-1}\times\left(\frac{A-x}{\left(\overline{A-x}^2+\frac{\overline{t-e}^2}{t^2}\times\overline{t^2}\right.}\right.$$

$$\left.-\frac{A-x}{\left(\overline{A-x}^2+t^2-x^2\right)^{\frac{n-1}{2}}}\right)=\frac{p}{n-1}\times\frac{\overline{n-1}\times\overline{A-x}\times\left(t-\frac{x^2}{t}\right)}{\left(A^2+t^2-2Ax\right)^{\frac{n+1}{2}}}\times e+\&c.\times e^2+$$

$$\&c.=p\times\left(t-\frac{x^2}{t}\right)\times\frac{A-x}{\left(A^2+t^2-2Ax\right)^{\frac{n+1}{2}}}\times e$$ nearly, which multiplied into $\dot{x}$, and the fluent of the reſulting fluxion found, it will be $p\times\left(A^2+t^2-2Ax\right)^{\frac{1-n}{2}}\times\left(\left(\frac{1}{\overline{n-7}\times tA}=d\right)x^3-\left(\frac{1}{\overline{n-5}t}+\right.\right.$
$\left.\frac{3d\times\overline{A^2+t^2}}{\overline{n-5}A}=c\right)x^2-\left(\frac{t}{\overline{n-3}A}+2c\times\frac{A^2+t^2}{\overline{n-3}\times A}=b\right)x+\frac{t}{n-1}-\left.\frac{b\times\overline{A^2+t^2}}{\overline{n-1}A}\right)$
e(M).

If the attraction of the whole ring contained between the ſphere and ſpheroid be required, ſubſtitute in the fluent (M) for x, t and $-t$, and proceed as in the preceding caſes; in like manner may be found the attraction of the ring contained between any two values of x.

18. The ſame principles may be applied to find the attraction of the above-mentioned ring, when the line Pp is not perpendicular to the circles pM, pm, CD, &c., and does not cut the diameters CD, M′M, &c. into two equal parts. They may be further applied for finding the attraction of rings contained between any other given ſolids, of which the equations differ by very ſmall quantities from each other; for example,

between two ſpheroids, of which the axes in the one do not differ much from correſpondent axes in the other, in which caſe the fluents found of the following fluxions may be uſeful.

$$1.\ \int\frac{\dot{x}}{(t^2-x^2)^{\frac{n-1}{2}}}=\frac{1}{\overline{n-3}\,t}\times\frac{x}{(t^2-x^2)^{\frac{n-3}{2}}}+\frac{n-4}{\overline{n-3}\,.\,\overline{n-5}\,t^4}\times\frac{x}{(t^2-x^2)^{\frac{n-}{}}}$$

$$+\frac{\overline{n-4}\,.\,\overline{n-6}}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,t^6}\times\frac{x}{(t^2-x^2)^{\frac{n-7}{2}}}+\frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\,t^8}$$

$$\frac{x}{(t^2-x^2)^{1}}\ \ldots\ \frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}\ldots 3}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\ldots 2t^{n-3}}\times\frac{x}{t^2-x^2}+$$

$$\frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}\ldots 3\,.\,1}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\ldots 2t^{n-3}}\times\int\frac{\dot{x}}{t^2-x^2}$$ if n be an odd number; but

$$2.\ \int\frac{\dot{x}}{\overline{t^2-x^2})^{\frac{n-1}{2}}}=\frac{1}{\overline{n-3}\,t^2}\times\frac{x}{(t^2-x^2)^{\frac{n-3}{2}}}+\frac{n-4}{\overline{n-3}\,.\,\overline{n-5}\,t^4}\times\frac{x}{(t^2-x^2)^{\frac{n-}{2}}}$$

$$+\frac{\overline{n-4}\,.\,\overline{n-6}}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,t^6}\times\frac{x}{(t^2-x^2)^{\frac{n-7}{2}}}\ \ldots\ldots+$$

$$\frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}\,.\,\overline{n-10}\ldots 4\,.\,2}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\,.\,\overline{n-11}\ldots 3\,.\,1t^{n-2}}\times\frac{x}{(t^2-x^2)^{\frac{1}{2}}},$$ if n be even.

Theſe principles may be applied to the finding approximations in very many philoſophical problems.

Cor. 1. From hence may be deduced the ſubſequent arithmetical theorems.

$$1.\ \frac{1}{2m-1}+\frac{2m-2}{\overline{2m-1}\,.\,\overline{2m-3}}+\frac{\overline{2m-2}\,.\,\overline{2m-4}}{\overline{2m-1}\,.\,\overline{2m-3}\,.\,\overline{2m-5}}+$$

$$\frac{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}}{\overline{2m-1}\,.\,\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}}\qquad\frac{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}\ldots 2}{\overline{2m-1}\,.\,\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}\ldots 3\,.\,1}$$

$= 1$; or, which is the ſame, $\frac{1}{2m-3}+\frac{2m-4}{\overline{2m-3}\,.\,\overline{2m-5}}+$

$2m-4$

$$\frac{\overline{2m-4}\times\overline{2m-6}}{\overline{2m-3}\times\overline{2m-5}\times\overline{2m-7}}\ldots\ldots+\frac{\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}\ldots4\times2}{\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\quad3\times1}$$

$$1\text{, or }\frac{1}{2m-5}+\frac{2m-6}{\overline{2m-5}\,.\,\overline{2m-7}}\qquad+\frac{\overline{2m-6}\,.\,\overline{2m-8}\ldots4\times2}{\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\ldots3\times1}$$

$=1$, &c.

$$\frac{}{\overline{2m-2}}+\frac{2m-3}{\overline{2m-2}\,.\,\overline{2m-4}}+\frac{\overline{2m-3}\,.\,\overline{2m-5}}{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}}+$$

$$\frac{\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}}{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}\,.\,2m\cdot}\ldots\ldots+$$

$$\frac{\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\ldots7\,.\,5}{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}\,.\,\overline{2m-10}\ldots6\,.\,4}+2\,;$$

$$\frac{\overline{2m-3}\,.\,\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\quad.\,3}{\overline{2m-2}\,.\,\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}\,.\,\overline{2m-10}\,.\,.\,2}=1\text{, or}$$

$$\frac{1}{2m-4}+\frac{2m-5}{\overline{2m-4}\,.\,\overline{2m-6}}+\frac{\overline{2m-5}\,.\,\overline{2m-7}}{\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}}\qquad+$$

$$\frac{\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\ldots5}{\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}\ldots4}+2\times$$

$$\frac{\overline{2m-5}\,.\,\overline{2m-7}\,.\,\overline{2m-9}\,.\,\overline{2m-11}\ldots5\,.\,3}{\overline{2m-4}\,.\,\overline{2m-6}\,.\,\overline{2m-8}\,.\,\overline{2m-10}\,.\,\overline{2m-12}\ldots4\,.\,2}=1\text{, \&c.}$$

3. By expanding the terms of the preceding fluents may be deduced the arithmetical theorems, *viz.*

$$\overline{n-4}\,.\,\overline{n-6}$$

$$\frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}}\times\overline{n-9}\,.\,\overline{n-7}\,.\,\overline{n-5}\,.\,\overline{n-3}\ldots\overline{n+m-6}$$

$$+\ldots\ldots+\frac{}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,n\cdot}\qquad\times2\,.\,4\,.\,6\,.\,8\ldots$$

$$\overline{m+5}+\frac{\overline{n-4}\,.\,\overline{n-6}\,.\,\overline{n-8}\,.\,\overline{n-10}\ldots3}{\overline{n-3}\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\,.\,\overline{n-11}\ldots2}\times2\times4\times6\times8\ldots$$

$$m+5$$

$$\overline{m+5}\times\frac{1}{m+6}=\overline{n-1}\cdot\overline{n+1}\cdot\overline{n+3}\cdot\overline{n+5}\ldots\overline{n+m+2}\times\frac{1}{m+6}$$

if n be an odd number.

2. $\frac{1}{n-2}\times\overline{n-3}\cdot\overline{n-1}\cdot\overline{n+1}\cdot\overline{n+3}\ldots\overline{n+2m+1}+$

$\frac{}{\overline{n-3}\cdot\overline{n-5}}\times\overline{n-5}\cdot\overline{n-3}\cdot\overline{n-1}\cdot\overline{n+1}\ldots\overline{n+2m-1}+$

$\frac{\overline{n-4}\cdot\overline{n-6}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}}\times\overline{n-7}\cdot\overline{n-5}\cdot\overline{n-3}\cdot\overline{n-1}\ldots\overline{n+2m-3}+$

$\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}}\times\overline{n-9}\cdot\overline{n-7}\cdot\overline{n-5}\cdot\overline{n-3}\ldots\overline{n+2m-5}$

$\ldots+\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}\ldots 2}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}\ldots 3\times 1}\times 1\cdot 3\cdot 5\cdot 7\ldots\overline{2m+5}=$

$\overline{n-1}\cdot\overline{n+1}\cdot\overline{n+3}\cdot\overline{n+5}\ldots\overline{n+2m+3}\times\frac{1}{2m+7}$, if n be an even number.

4. Arithmetical theorems, ſomewhat different, may be deduced from taking the fluxions of the preceding fluents, and reducing the fractions reſulting to a common denominator by multiplying them into t^2-x^2, $(t^2-x^2)^2$, $(t^2-x^2)^3$, &c.

1. $\frac{1}{n-3}\times\overline{n-4}+\frac{n-4}{\overline{n-3}\cdot\overline{n-5}}\times\overline{n-7}+\frac{\overline{n-4}\cdot\overline{n-6}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}}\times\overline{n-10}$

$+\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}}\times\overline{n-13}\ldots+\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}\ldots 3}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}\ldots 2}$

$\times\frac{7-n}{2}-\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}\cdot\overline{n-10}\ldots 3}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}\cdot\overline{n-11}\ldots 2}\times\frac{n-3}{2}=0$, if n be odd.

2. $\frac{1}{n-3}\times\overline{n-4}+\frac{n-4}{\overline{n-3}\cdot\overline{n-5}}\times\overline{n-7}+\frac{\overline{n-4}\cdot\overline{n-6}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}}\times\overline{n-10}$

$+\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}}\times\overline{n-13}\ldots+\frac{\overline{n-4}\cdot\overline{n-6}\cdot\overline{n-8}\ldots 2}{\overline{n-3}\cdot\overline{n-5}\cdot\overline{n-7}\cdot\overline{n-9}\ldots 3\times 1}$

$\times\frac{4-n}{2}=0$, if n be even; or more general, if n be even,

3.

$$3.\ \frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots\overline{n-2m-2}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots\overline{n-2m-3}}\times\overline{n-2m-4}+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}.\overline{n-10}\ldots\overline{n-2m-4}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}.\overline{n-11}\ldots\overline{n-2m-5}}\times(\overline{m+1}\times\overline{n-2m-6}-1)$$

$$+\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}.\overline{n-10}.\overline{n-12}\ldots\overline{n-2m-6}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}.\overline{n-11}.\overline{n-13}\ldots\overline{n-2m-7}}\times$$

$$(\overline{m+1}.\frac{m+2}{2}\times\overline{n-2m-8}-\overline{m+2})+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}.\overline{n-10}.\overline{n-12}.\overline{n-14}\ldots\overline{n-2m-8}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}.\overline{n-11}.\overline{n-13}.\overline{n-15}\ldots\overline{n-2m-9}}$$

$$\times(\overline{m+1}.\frac{m+2}{2}.\frac{m+3}{3}\times\overline{n-2m-10}-\overline{m+2}.\frac{m+3}{2})+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}.\overline{n-10}\ldots\overline{n-2m-10}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}.\overline{n-11}\ldots\overline{n-2m-11}}\times(\overline{m+1}.\frac{m+2}{2}.\frac{m+3}{3}.$$

$$\frac{m+4}{4}\times\overline{n-2m-12}-\overline{m+2}.\frac{m+3}{2}.\frac{m+4}{3})\ldots\ldots+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots 2}{\overline{n-3}.\overline{n-5}.\overline{n-7}\ldots 3\times 1}\times-\overline{m+2}.\frac{m+3}{2}.\frac{m+4}{3}\ldots\ldots\frac{n-4}{n-2m-6}$$

$$=0.$$

$$4.\ \frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots\overline{n-2m-2}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots\overline{n-2m-3}}\times\overline{n-2m-4}+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots\overline{n-2m-4}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots\overline{n-2m-5}}\times(\overline{m+1}\times\overline{n-2m-6}-1)+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots\overline{n-2m-6}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots\overline{n-2m-7}}\times(\overline{m+1}.\frac{m+2}{2}\times\overline{n-2m-8}-$$

$$\overline{m+2})+\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots\overline{n-2m-8}}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots\overline{n-2m-9}}\times(\overline{m+1}.\frac{m+2}{2}\times$$

$$\frac{m+3}{3}\times\overline{n-2m-10}-\overline{m+2}.\frac{m+3}{2})+\ldots\ldots\ldots+$$

$$\frac{\overline{n-4}.\overline{n-6}.\overline{n-8}\ldots 5\times 3}{\overline{n-3}.\overline{n-5}.\overline{n-7}.\overline{n-9}\ldots 4\times 2}\times(\overline{m+1}.\frac{m+2}{2}.\frac{m+3}{3}\ldots\frac{n-5}{n-2m-5}-$$

$$\overline{m+2}.\frac{m+3}{2}.\frac{m+4}{3}\ldots\frac{n-5}{n-2m-7})-$$

$$\frac{n-4\,.\,n-6\,.\,n-8\,\ldots\,5\times3}{3\,.\,\overline{n-5}\,.\,\overline{n-7}\,.\,\overline{n-9}\,\ldots\,4\times2}\times\overline{m+2}\,.\,\frac{m+3}{2}\times\frac{m+4}{3}$$

$_{n-2m-5}$ = 0, if *n* be an odd number.

Many more arithmetical theorems may be deduced from the fluents of theſe and other fluxions by ſimilar methods, which cannot, without ſome difficulty, be found from the common methods of finding the ſums of ſeries.

This method of finding approximations to the areas and lengths of curves, fluents of fluxions, and ſums of ſeries, &c. of which the equations, to their increments, fluxions, &c. are given from the areas of curves, fluents, &c. of which the equations to their increments, &c. differ by very ſmall quantities from the given equations was publiſhed in the Meditat. Analyt. near twenty years ago.

I ſhall conclude this Paper with two theorems of ſome little uſe in the doctrine of chances.

THEOREM I.

$$H=\overline{a+b}\times\overline{a+b-1}\,.\,\overline{a+b-2}\,.\,\overline{a+b-3}\,\ldots\,\overline{a+b-n+1}$$

$$=a\,.\,\overline{a-1}\,.\,\overline{a-2}\,.\,.\,\overline{a-n+1}+n\,.\,a\,.\,\overline{a-1}\,.\,\overline{a-2}\,.\,.\,\overline{a\quad n+2}$$

$$\times b+n\,.\,\frac{n-1}{2}\times a\,.\,\overline{a-1}\,.\,\overline{a-2}\,\ldots\,\overline{a-n+3}\times b\,.\,\overline{b-1}+n\,.$$

$$\frac{n-1}{2}\,.\,\frac{n-2}{3}\,a\,.\,a-1\,.\,a-2\,.\,.\,a-n+4\times b\times b-1\,.\,b-2+$$

$$.\,.\,.\,.\,.+n\,.\,\frac{n-1}{2}\,.\,\frac{n-2}{3}\,.\,\frac{n-3}{4}\,.\,.\,.\,.\,\frac{n-l+1}{l}\,(L)\,a\,.\,\overline{a-1}\,.\,\overline{a-2}$$

$$.\,\overline{a-3}\,.\,.\,.\,\overline{u-n+l+1}\times b\,.\,\overline{b-1}\,.\,\overline{b-2}\,.\,.\,.\,\overline{b-l+1}+\,.\,.\,.+$$

$n\,.\,\frac{n-1}{2}\,a\,.\,\overline{a-1}\,.\,b\,.\,\overline{b-1}\,.\,\overline{b-2}\,.\,.\,\overline{b-n+3}+na\,.\,b\,.\,\overline{b-1}\,.\,\overline{b-2}\,.\,.\,.\,\overline{b-n+2}+b\,.\,\overline{b-1}\,.\,\overline{b-2}\,.\,.\,.\,\overline{b-n+1}$.

If for $a+b-1$, $a+b-2$, $a+b-3$, &c., $a-1$, $a-2$, &c., $b-1$, $b-2$, &c. be ſubſtituted reſpectively $a+b-x$, $a+b-2x$, $a+b-3x$, &c., $a-x$, $a-2x$, $a-3x$, &c., $b-x$, $b-2x$, $b-3x$, &c., the reſulting equation will equally be juſt; and, laſtly, if for x be ſubſtituted 0, it will become the binomial theorem.

Cor. If there are two different events A and B, of which the numbers are reſpectively a and b, and their chances of happening alſo as a and b; and if A's happen, let the whole number $(a+b)$ and alſo the number of A's be diminiſhed by x, and in the ſame manner of B's happening, and ſo on; then will the chance of A's happening $n-l$ times, and B's happening l times in n trials be $\mathrm{L}\times a\,.\,\overline{a-x}\,.\,\overline{a-2x}\,.\,.\,\overline{a-(n-l-1)x}\times b\,.\,\overline{b-x}\,.\,\overline{b-2x}\,.\,.\,\overline{b-(l-1)x}$ divided by H.

In a ſimilar manner may be found, 1. the chance of A's happening between h and k times; and, 2. the chance of A's happening (h) to B's happening (k) times; 3. of A's and B's happening reſpectively h and k times more than the other; 4. the chance of A's happening an even to its happening an odd number of times, &c. in (n) trials, &c. &c. &c.

THEOREM II.

$\mathrm{H}=\overline{a+b+c+d+\&c.}\times\overline{a+b+c+d+\&c.-x}\times\overline{a+b+c+d+\&c.-2x}\,.\,.\,.\,.\,\overline{a+b+c+d+\&c.-(n-1)x}=a\,.\,\overline{a-x}\,.\,\overline{a-2x}\,.\,.\,.\,\overline{a-\overline{n-1}x}+n\,.\,a\,.\,\overline{a-x}\,.\,\overline{a-2x}\,.\,.\,\overline{a-\overline{n-2}x}\times b+c+d+\&c.$

$$+ n . \frac{n-1}{2} . a . \overline{a-x} . \overline{a-2x} .. \overline{a-\overline{n-3}x} \times (b . \overline{b-x} + c \times \overline{c-x} + d . \overline{d-x} + \&c. + 2bc + 2bd + 2cd + \&c.) + ... + \mathrm{L} \times (a . \overline{a-x} . \overline{a-2x} ... \overline{a-\overline{l-1}x} \times b . \overline{b-x} . \overline{b-2x} .. \overline{b-\overline{m-1}x} \times c . \overline{c-x} . \overline{c-2x} .. \overline{c-\overline{p-1}x} \times d . \overline{d-x} . \overline{d-2x} ... \overline{d-\overline{q-1}x} \times \&c. = \mathrm{K}) + \&c.,$$

where

$$\mathrm{L} = n . \frac{n-1}{2} . \frac{n-2}{3} .. \frac{n-l+1}{l} \times \overline{n-l} . \frac{n-l-1}{2} . \frac{n-l-2}{3} .. \frac{n-l-m+1}{m} \times \overline{n-l-m} . \frac{n-l-m-1}{2} . \frac{n-l-m-2}{3} ... \frac{n-l-m-p+1}{p} \times \overline{n-l-m-p} . \frac{n-l-m-p-1}{2} \frac{n-l-m-p-2}{3} .. \frac{n-l-m-p-q+1}{q} \times \&c.$$

which is the ſame as the co-efficient of the term $a^l \times b^m \times c^p \times d^q \times$ &c. in the multinomial $a + b + c + d +$ &c. raiſed to the power n.

The chance of any number of events A, B, C, D, &c. of which the numbers are a, b, c, d, &c. happening l, m, p, q, &c. times reſpectively in a ſimilar manner to A's and B's happening in the preceding caſe will be $\frac{\mathrm{L} \times \mathrm{K}}{\mathrm{H}}$.

All the propoſitions mentioned as immediately deducible from the preceding theorem may, *mutatis mutandis*, with the ſame eaſe be applied to more events A, B, C, D, &c.

If for a, b, c, d, &c. be ſubſtituted the ſame letters, increaſed or diminiſhed by any given quantities, the reſulting equation will be equally true.

ERRATA.

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Page 171. *after* $+n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\log.\overline{r-3p}+$&c. *is omitted* but log. $r-n\times$ log. $\overline{r-p}+n\cdot\frac{n-1}{2}\log.\overline{r-2p}-n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\times\log.\overline{r-3p}+$ &c. $=$, which probably happened, in my abſence from the preſs, on account of the ſimilarity of the preceding quantity.

Page 172. l. 1. *for* $(\pm M+$ *read* $+(M\pm$

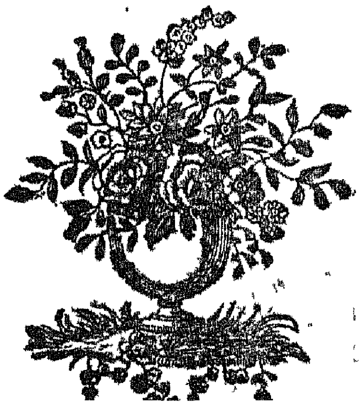


Fig 1.

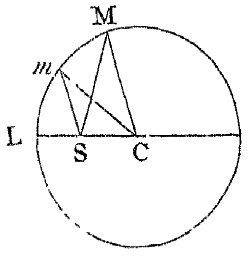
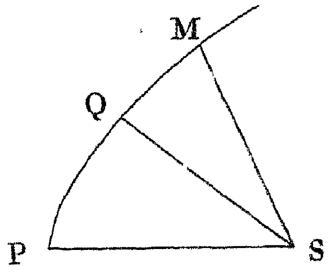


Fig 2.



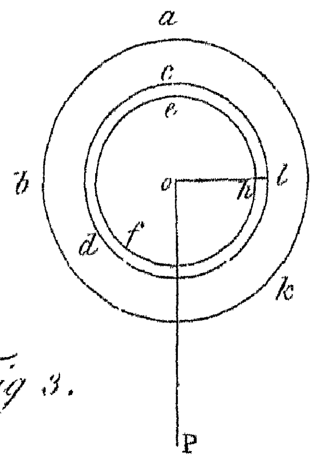


Fig 3.

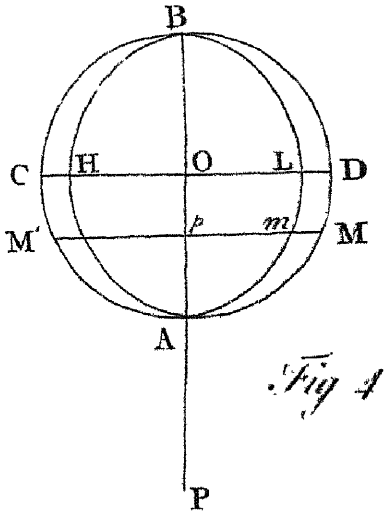


Fig 4.

X. *An Account of ſome Appearances attending the Converſion of caſt into malleable Iron. In a Letter from* Thomas Beddoes, *M. D. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 24, 1791.

SIR,

YOU are undoubtedly well apprized of an alteration lately introduced into our manufactories of iron, in conſequence of which the reverberatory has been ſubſtituted in the place of the finery furnace. The new proceſs is capable of being indefinitely varied. I have lately been favoured with an opportunity of obſerving one of theſe variations with every advantage I could deſire. As in this method the changes undergone by the metal during the firſt ſeries of operations lie perfectly open to inſpection, a ſhort deſcription of them may not perhaps be unworthy the notice of philoſophical chemiſts. Allow me to premiſe further, that I did not content myſelf with a ſingle examination; and, for the ſake of greater accuracy, I took minutes of the phænomena, and of the time when they occurred. A very intelligent workman was at the ſame time directed to anſwer all my queſtions, ſo that I enjoyed the benefit of his experience alſo.

In ſomewhat more than half an hour after it was put in, the charge conſiſting of 2½ cwt. of grey pig iron was nearly melted. The workman now began to ſtir the liquid maſs:

for this purpoſe he uſed ſometimes an iron lever, and ſometimes a kind of hoe; but he firſt turned the flame from off the metal, which is done by letting down a damper upon the chimney correſponding to that with which ordinary reverberatory furnaces are provided, and by raiſing the damper of a ſecond chimney, which proceeds immediately from the fire-place, and carries off the flame, current of air, &c. without allowing it paſs into the body of the furnace.

In 50 minutes from the commencement of the operation, the metal had become in conſequence of the conſtant ſtirring looſe and incoherent; it appeared about as ſmall as gravel; it was now alſo ſtiff, and much cooled.

55 m. from the ſame period, flame turned on again. Workman keeps ſtirring and turning over the metal; in 3 m. it becomes ſoft and ſemi-fluid; flame turned off; the hotteſt part of the maſs begins to heave and ſwell, emitting a deep blue lambent flame. The workman calls this appearance fermentation.

1 hour 1 m. blue flame breaking out over the whole maſs; heaving motion alſo general.

1 h. 13 m. metal full as hot, or, as the workman and myſelf both judged, rather hotter than at the inſtant the flame was turned off, though it is now a quarter of an hour ſince.

1 h. 18 m. where there is no heaving and no blue flame the maſs is ſenſibly cooler, and only of a dull red heat.

1 h. 20 m. workman obſerves, that the metal ſticks leſs to his tools. Pig-iron, he ſays, faſtens upon it immediately, and muſt be ſhaken off by ſtriking the other end with an hammer; as it approaches more and more towards *nature* (malleable iron) it adheres leſs; and when the tools come clear up out of the maſs. he judges it to be fermented enough.

1 h. 23 m. little heaving or blue flame; metal ſtiffer, and of a dull red; flame turned on and ſoon off again.

1 h. 26 m. by conſtant ſtirring the metal is become as fine as ſand. Workman remarks, that the flame, which re-appears over the whole maſs, looks more kindly. It is evidently of a lighter blue colour.

1½ h. flame turned on and ſoon off again. Maſs ferments ſtrongly. Hiſſing noiſe heard: this noiſe was diſtinguiſhable in ſome degree ever ſince the blue flame and heaving motion became viſible, but always faint till now.

1 h. 40 m. leſs blue flame.

1 h. 48 m. flame twice turned on and off in this interval. Metal now clots, ſtands wherever it is placed, without any tendency to flow, and no liquid pig iron now remains in the baſon of the furnace; the maſs has been conſtantly ſtirred and turned over.

1 h. 50 m. a little finery cinder appears boiling up amid the maſs. Workman attributes the increaſe of the hiſſing to this.

1 h. 53 m. ſcarce any perceptible blue flame or heaving. All the metal is now gathered into lumps, which the workman beats and preſſes with an heavy-headed tool. He brings them ſucceſſively into the hotteſt part of the furnace, into which the flame has been admitted. He now ſtops the port hole in the door at which he had introduced his tools, and applies a fierce flame for 6 or 8 minutes; the metal is then rolled.

Theſe appearances, at leaſt the moſt intereſting of them, ſeem to admit of an eaſy explanation; and I offer the following obſervations as ſupplemental to thoſe for which we are already indebted to the Swediſh and French chemiſts on this important branch of metallurgy. I aſſume the following propoſitions as already proved by theſe philoſophers. 1. That caſt iron is iron imperfectly reduced, or, in other words, that it contains a portion of the baſis of vital air, the oxygène of M. LAVOISIER. 2. That it contains a portion of plumbago, with which grey caſt iron moſt abounds. 3. That plumbago conſiſts of iron united to charcoal. 4. That fixed air, which I would rather call carbonic acid air, conſiſts of oxygène and the conſtituent parts of charcoal.

The heaving or ſwelling motion, ſo conſpicuous in the proceſs, is doubtleſs owing to the diſcharge of an elaſtic fluid; and the lambent deep blue flame, breaking out in ſpots over the whole ſurface, ſhews, that this elaſtic fluid is an inflammable gas of the heavy kind. That no doubt might be left upon the former of theſe circumſtances, I directed the workman to take out, at two different periods, a quantity of the metal where it was working moſt ſtrongly. Both proved, on examination, to be ſpungy, cellular, and full of bladder holes.

The heavy inflammable air, I imagine, is produced in this manner. The oxygène of the imperfectly reduced metal combines with the charcoal to form fixed air; at the ſame time another portion of charcoal is thrown into an elaſtic ſtate, that is, into inflammable air, and burns on the ſurface with a very deep blue flame, on account of the admixture of fixed air. The heat which is ſo obviouſly generated in the maſs at the beginning of the fermentation, I attribute to the combination of the oxygène and charcoal; a fact which, with ſeveral others as I have already remarked on another occaſion *, ſhews, if not the falſehood, at leaſt the imperfection of the modern doctrine on the ſubject of heat. The acidifying principle, it would appear, has ſome power of generating heat independant of its condenſation. Here abundance of elaſtic matter is

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* Chemical opinions of a philoſopher of the laſt century.

diſcharged; yet, notwithſtanding the heat abſorbed by its formation, and that which flows out of the metal in all directions, the whole maſs becomes hotter. The oxygène cannot be ſuppoſed to have much ſpecific or latent heat, becauſe it undoubtedly exiſts in the iron in a very condenſed ſtate. Neither does the appearance of the maſs allow me to aſcribe this generation of heat to the burning of the inflammable air at the ſurface, as will alſo be immediately evident for another reaſon. The leſs deep blue colour of the flame at a ſubſequent period in the operation is probably owing to the abſence of fixed air, or at leaſt to its being produced more ſparingly, the oxygène being now nearly conſumed. It will not appear ſurprizing, that the oxygène in this caſe ſhould be conſumed before the charcoal, if it be conſidered, 1. that grey iron contains a large portion of plumbago; and, 2. that fixed air contains a much larger quantity of oxygène than of charcoal; near three times as much, according to our beſt experiments on its formation: ſo that I aſcribe the ſubſequent fermentation accompanied with the lighter coloured flame almoſt entirely to the converſion of the charcoal into an elaſtic fluid. A very experienced philoſopher, I am well aware, has aſſerted, that water is neceſſary to this converſion; an opinion concerning the juſtneſs of which I have long entertained great doubts. Whenever I have diſtilled charcoal *per ſe*, I have found the firſt portions of gas to contain fixed air; an appearance owing, as I believe, to the decompoſition of water abſorbed from the atmoſphere; but, after continuing the proceſs for ſome time, there has ſtill been a production of inflammable air; but from this neither lime-water nor milk of lime would abſorb any portion, though when fired with vital or common air, it would produce fixed air; and if moiſture was added to the charcoal, inflammable and fixed air would be generated anew. Moreover it appears, from the experiments of Dr. Austin and ſome others, that charcoal conſiſts of the hydrogène and azote of the French chemiſts. How far it may be difficult or impoſſible entirely to convert charcoal in its ordinary ſtate into gas, is a point I wiſh to ſee more fully illuſtrated by future experiments. At preſent it ſeems obvious, that the circumſtances of the operation I have deſcribed are particularly favourable to this converſion: for, 1. not to mention the violence of the heat, we have this ſubſtance in a very attenuated ſtate, ſo that, very probably, the expanſive power of fire is very little, if at all, counteracted by the attraction of coheſion, which cannot be ſaid in the caſe of the moſt minute mechanical diviſion we can effect. 2. The attraction of the particles of the iron for one another will produce an effort to extrude the intermixed particles of charcoal, and thus enable it more readily to aſſume the elaſtic form.

Now, during the continuance of the lighter coloured blue flame, the maſs, as I obſerved, ſhews no power of generating heat within itſelf; a circumſtance which indicates that the heat produced in the former part of the operation does not depend on the burning of the gas at the ſurface; and I think inſpection will ſatisfy any one that it is produced in the heart of the maſs. It may indeed be objected, that the metal, now brought nearer to the ſtate of malleable iron, may require a greater ſupply of heat to keep it at the ſame temperature. It is leſs fuſible, as we are well aſſured. By referring back to the minutes you will obſerve, how very often it was neceſſary to turn the flame upon the maſs during this ſecond fermentation in order to keep it in a ſtate in which it could be worked.

The very copious production of elaſtic fluids during an hour, and often during a much longer ſpace, for in this inſtance the proceſs was remarkably ſucceſsful and ſhort, does not ſeem favourable to a late ingenious hypotheſis, according to which water is the embodying principle of all elaſtic fluids. I have never indeed conſidered this as very probable, and, after the obſervations I have related, I ſee no means of defending it. Will it be ſaid, that the pig iron, as being in ſome ſort a calx of iron, contains water?

In annealing crude iron, with or without charcoal, it is well known to increaſe in all its dimenſions. I have ſeen bars originally ſtraight bent like an S, when long expoſed to heat in circumſtances where they could not extend themſelves endways. I ſuppoſe this phænomenon may be owing to a very ſmall beginning of this fermentative motion, which acts as an internal principle of expanſion. Caſt iron bars, not in contact with charcoal, would, according to this ſuppoſition, by long annealing loſe of their weight; or if the heat was too low for the elaſtic fluid to be diſcharged from their ſubſtance, they would probably bliſter like ſteel: an appearance undoubtedly owing to the generation of air. Mr. HORNE, in his Eſſay on Iron, ſomewhere remarks, that on opening theſe bliſters he has heard a whiſtling noiſe as of air ruſhing out.

During the whole of this proceſs, frequent jets of white ſparks, of a dazzling brightneſs, played from the ſurface of the metal. They would have afforded an extremely beautiful ſpectacle but for the inconvenience of looking on ſo hot a maſs. They aroſe, no doubt, from the burning of ſmall portions of iron.

The effect of ſo much ſtirring as I have noted down does not require to be explained.

The workman was clearly of opinion, that the fermentation of hard or white crude iron is leſs than of grey in this proceſs; a fact which perfectly coincides with the preceding obſervations, ſince that ſpecies contains leſs plumbago, or in other words leſs matter fit to produce elaſtic fluids.

In order to prove the extrication of fixed air during the fermentation of the metal, I once thought of introducing lime-water in an iron veſſel within the body of the furnace; but when I conſidered that the fire-place was not divided by any partition from the body of the furnace, and that the whole building was full of burned air, I omitted the experiment from a perſuaſion that, even if the lime-water ſhould become turbid, the fixed air might come from another ſource.

I was not unmindful of the ſulphur which exiſts, as I have reaſon to believe, in every form of iron manufactured with coaks. I cannot, however, aſcribe any of the effects I obſerved to its preſence. There can be little doubt, that ſome portion was perpetually extricated with the inflammable air during the whole proceſs; for on diſſolving pieces of the ſtamped, or rather the rolled iron in weak muriatic acid, ſilver held in the extricated air was tarniſhed as much and as ſoon as by air from ſpecimens taken out of the furnace at different times during the proceſs. I could not but conclude, that the tarniſhing matter came from the iron, when I found the air from a ſolution of zinc in the ſame acid, incapable of producing the colour upon ſilver. The appearance, the want of a martial aſtringent taſte, and the diſſolving action of cauſtic alkali, led me to conclude, that the colour in each experiment with iron was derived from ſulphur.

I leave it to the adherents of phlogiſton to accommodate theſe phænomena to their doctrine; conſidering it, for my own part, as ſuperfluous to beſtow any further attention upon a ſyſtem which, after a long diſcuſſion, has been fully refuted in all its modifications, and which indeed ſeems on the eve of being univerſally abandoned.

I have the honour to be, &c.

THOMAS BEDDOES.

XI. *On the Decompoſition of Fixed Air.*

By Smithſon Tennant, *Eſq. F. R. S.*

Read March 31, 1791.

AS fixed air is produced by the combuſtion of charcoal, it has long been thought highly probable that vital air and charcoal are its conſtituent ingredients. This opinion is confirmed by the experiments of M. LAVOISIER, from which he diſcovered that the weight of the fixed air which is formed during the combuſtion is nearly equal to that of the vital air and charcoal conſumed in the proceſs; and that the ſmall difference of weight may, with great reaſon, be attributed to the production of water ariſing from inflammable air contained in the charcoal. The compoſition of fixed air therefore ſeems to be determined, by uniting its conſtituent parts, with as much certainty as by that mode of proof alone it is poſſible to obtain. But as vital air has a ſtronger attraction for charcoal than for any other known ſubſtance, the decompoſition of fixed air has not hitherto been attempted. By means, however, of the united force of *two* attractions I have been able to decompoſe fixed air, and thus to determine its conſtituent parts in conſequence of their ſeparation.

It has long been known, that when phoſphoric acid is combined with calcareous earth, it cannot be decompoſed by diſtillation with charcoal: for though vital air is more ſtrongly attracted by charcoal than by phoſphorus, yet in this compound it is retained by two attractions, by that which it has for phoſphorus, and by that which the phoſphoric acid has for lime, ſince the vital air cannot be diſengaged unleſs both theſe attractions are overcome. As theſe attractions are more powerful than that which charcoal has for vital air, if phoſphorus is applied to fixed air and calcareous earth, the vital air will unite with the phoſphorus, and the charcoal will be obtained pure. Theſe ſubſtances, in order to act upon each other, muſt be brought into contact when red-hot; and this may be eaſily effected in the following manner. Into a glaſs tube, cloſed at one end, and coated with ſand and clay to prevent the ſudden action of the heat, a little phoſphorus ſhould be firſt introduced, and afterwards ſome powdered marble. The experiment ſucceeds more readily if the marble is ſlightly calcined, probably becauſe that part which is reduced to lime, by immediately uniting with the phoſphorus, detains it to act upon the fixed air in the other part. After the ingredients are introduced, the tube ſhould be nearly, but not entirely, cloſed up; by which means ſo free a circulation of air as might inflame the phoſphorus is prevented, whilſt the heated air within the tube is ſuffered to eſcape. When the tube has remained red-hot for ſome minutes, it may be taken from the fire, and muſt be ſuffered to grow cold before it is broken. It will be found to contain a black powder, conſiſting of charcoal intermixed with a compound of lime and phoſphoric acid, and of lime united with phoſphorus. The lime and phoſphoric acid may be ſeparated by ſolution in an acid and by filtration, and the phoſphorus by ſublimation.

Charcoal, thus obtained from fixed air, appears in no reſpect to differ from the charcoal of vegetable matters. On deflagrating a little of it in a ſmall retort with nitre, fixed air was immediately reproduced.—Since, therefore, charcoal, by its ſeparation from fixed air, is proved to be one of its conſtituent principles, it can hardly be doubted, that this ſubſtance is preſent whenever fixed air is produced; and that thoſe experiments, from which it is ſuppoſed that this acid may be formed without the aid of charcoal, have not been conducted with the requiſite caution.

As vital air is attracted by a compound of phoſphorus and calcareous earth more powerfully than by charcoal, I was deſirous of trying their efficacy upon theſe acids, which may from analogy be ſuppoſed to contain vital air, but which are not affected by the application of charcoal. With this intention I made phoſphorus paſs through a compound of marine acid and calcareous earth, and alſo of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the ſtrong attraction which theſe acids have for calcareous earth tends to prevent their decompoſition, it might be thought that in this manner they were not more diſpoſed to part with vital air than by the attraction of charcoal. But this, however, does not appear to be the fact. I have found, that phoſphorus cannot be obtained by paſſing marine acid through a compound of bones and charcoal, when red-hot. The attraction, therefore, of phoſphorus and lime for vital air exceeds the attraction of charcoal by a greater force than that ariſing from the attraction of marine acid for lime.

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XII. *A Meteorological Journal, principally relating to Atmoſpheric Electricity; kept at* Knightſbridge, *from the 9th of* May, 1789, *to the 8th of* May, 1790. *By Mr.* John Read; *communicated by* R. H. A. Bennet, *Eſq. F. R. S.*

Read April 14, 1791.

A DESCRIPTION of the inſtrument for collecting atmoſpheric electricity, uſed in the following journal.

Tab. V. repreſents the apparatus. AA is a round deal rod, 20 feet long, 2 inches diameter at the lower, and one inch at the upper end. Into the lower end of it is cemented a ſolid glaſs pillar B, 22 inches long; the lower end of the glaſs ſtands in a hole made for it in a pedeſtal of wood C, which ſlips on the fore-part of an iron bracket D, which is driven into the wall, and ſupports the whole. About 13 feet above the bracket D, is fixed to the wall a ſtrong arm of wood E, which holds perpendicularly a ſtrong glaſs tube F, through which the rod is ſlided gently upwards, till the glaſs pillar B may be lowered into the hole made for it in C. It is thus fixed, and ſtands 12 inches from the wall. The tube F is of ſufficient width to admit a caſe of cork, which is faſtened in the inſide of it, at the part where the tube is ſuſtained by the arm of wood E, ſo that the rod, when bent by the wind, cannot touch the tube or break it. The upper extremity of the rod is terminated by ſeveral ſharp-pointed wires G. Two of them are of copper, each one-eighth of an inch thick; and, in order to ſtiffen the rod, as well as conduct more readily the electric fluid, one of thoſe wires is twiſted round the rod to the right hand, and the other to the left, as low down as the braſs collar at the vertex of the lower funnel H, to which they are ſoldered, in order to render their contact perfect. The tin funnels HH ſerve to defend the glaſſes B and F from the weather, which glaſſes are alſo covered with ſealing-wax to render their inſulation more perfect. At a convenient height from the floor, a hole is bored through the wall at I. This hole receives a glaſs tube covered with ſealing-wax, through which a ſtrong braſs wire proceeding from the rod is conveyed into the room, where juſt at the end of the glaſs tube it paſſes through a two-inch braſs ball L, and proceeding a little farther, keeps ſuſpended at its extremity a pith ball electrometer K, ſo that the electrometer may be about twelve inches diſtant from the wall. On the outſide of the wall there is a wooden box M, to keep that end of the glaſs tube dry.

At two inches diſtance of the above-mentioned braſs ball L, a bell N is ſupported by a ſtrong wire, which paſſing through another hole made in the wall, is made to communicate, by means of a good metallic continuation R, with the moiſt ground adjoining to the houſe. A braſs ball, three-tenths of an inch in diameter, is ſuſpended between the bell N and ball L, by a ſilk thread faſtened to a nail O. This ball ſerves for a clapper, by ſtriking between the ball and bell, when the electrical charge of the rod is ſufficiently ſtrong.

P is a ſmall table fixed to the wall under the bell and ball, at a convenient height above the floor; upon which Leyden bottles and other apparatus are occaſionally placed. Any perſon verſed in the ſcience of electricity, will eaſily underſtand that this apparatus is calculated to ſhew the various degrees of atmoſpherical electricity, and at the ſame time to avoid the pernicious effects which may be occaſioned by thunder-ſtorms, or in ſhort by any great quantity of electricity in the atmoſphere.

The whole perpendicular height of both parts taken together, from the moiſt earth to the uppermoſt point at the top of the rod, is 52 feet.

Finding, however, that, notwithſtanding all the precaution I had taken to procure a good inſulation, the moiſt vapour of the atmoſphere, fixing upon the inſulating parts of the apparatus, rendered it imperfect in moiſt weather; I have lately (15th of Sept. 1790) altered the ſituation of the ſame rod, ſo that all the inſulating parts are now within the roof of the houſe. This I have effected by a hole through the roof of my houſe; by which means I now obtain a conſiderably more conſtant electricity; which, however, muſt not be ſolely attributed to the ſuperiority of my preſent mode of inſulating, but to the rod's being alſo elevated to the additional height of nine feet; ſo that I conſider its pointed part to be at preſent 61 feet above the moiſt earth.

This improvement of the apparatus, having been made after the concluſion of this journal, will be particularly deſcribed in the next, which I am now carefully continuing.

It will be neceſſary juſt to mention the method I have purſued in forming the journal of atmoſpheric electricity. This has been principally by means of the ſigns exhibited by the pith balls K, connected with the rod. When I find theſe cloſed, and not attracted by my finger, I then write no ſigns of electricity. When attracted on the approach of my finger, yet not ſufficiently charged to repel each other, I write weak ſigns of the fluid. When I find the balls open, and, on the approach of excited glaſs, the balls cloſe, I write they are electrified poſitively;

tively; but, if the balls open wider, I write they are electrified negatively; and the reverſe when I uſe ſealing-wax. When the balls diverge one inch and upwards, viſible ſparks may be drawn at the braſs ball L. When ſparks are ſaid to have been perceived in any obſervation, I have generally on that account omitted to note the variable quantities of divergency in the pith balls. Their utmoſt limit of regular divergency ſeems to be about five or near ſix inches; above that they are unſteady and diſorderly. The pith balls are near two-tenths of an inch in diameter, ſuſpended by very fine flaxen threads (in the ſtate it is in from the heckle) five inches long. When I mention the diſtance of the balls in tenths of an inch, it is to be underſtood as nearly ſo as my eye can determine.

This apparatus requires a conſtant attention, eſpecially during a diſturbed ſtate of the atmoſphere. From the room in which the apparatus is placed I am ſeldom abſent one hour, excepting the time of ſleep; but, when I leave it, the laſt thing I do at night is to examine the ſtate of the electricity, and, if I find the rod unelectrified, I then place the Leyden bottle on the table P, with its knob nearly in contact with the ball L. The next morning, if I find this bottle charged, I write the kind of electricity it is charged with againſt the day in the journal, and add, *by the night bottle*.

It is preſumed, that the table is ſufficiently obvious. The two columns for poſitive and negative electricity are uſed only for the firſt obſervation of each day. I uſe FAHRENHEIT's thermometer, ſuſpended on the north outſide of a bow window. The time of making the obſervation with it, and the barometer, and alſo of the direction of the wind, has uſually been about nine o'clock in the morning.

Laſtly, it may be uſeful to obſerve, that I have always found the lower though uninſulated part of the apparatus (*viz.*

the metallic connection of the bell N with the moiſt earth) to be in a contrary ſtate of electricity to the upper and inſulated part, where the pith balls K are ſuſpended. See the 22d of Aug.

Having made a memorandum of the ſeveral thunder-ſtorms which have happened in divers parts of this iſland, according to the information by letters, and from news-papers, I thought it uſeful to inſert them in this journal, in order to ſhew whether ſome contemporaneous appearances in my apparatus might not be attributed to them. This ſeems evidently to have been the caſe on the 3d of September.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	May 9, 1789.
		Inches.	°				
May 9	NW	30.	63	o	—	neg.	Balls open about three-tenths of an inch.
10	SW	30.15	60	o	—	neg.	But very weak.
11	E	30.24	61	o	—	neg.	Barely ſufficient to ſeparate the balls.
12	E	29.91	60	ſmall	—	neg.	The firſt day I have had viſible ſparks; weather cloudy, but fair.
13	E	29.91	61	ſtrong	poſ.	—	The weather in the forenoon a little hazy; in the afternoon a thick fog; four o'clock a little rain fell; the rod now became highly electrified poſitively; the bell rang briſkly. I now filled ſeveral bottles with the fluid. This ſtrong charge in the rod did not laſt longer than one hour, but it remained charged poſitively in a leſs degree the reſt of the day. There was this day ſome lightning and thunder at Saliſbury, and to the weſt of it.
14	SE	30.	56	o	poſ.	—	Balls open four-tenths of an inch.
15	E	29.66	58	ſmall	poſ.	—	A.M. and P.M. negative.
16	SE	29.83	61	o	—	neg.	A.M. and P.M.
17	S	29.91	58	o	—	neg.	Balls open ſix-tenths of an inch.
18	SW	29.91	57	o	—	neg.	Balls open five-tenths of an inch.
19	N	30.20	54	o	—	neg.	P.M. dark heavy weather.
20	SE	30.15	56	ſmall	—	neg.	The rod was electrified nearly the whole day.
21	E	30. 2	54	o	—	neg.	Nearly the ſame as the preceding day.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
May 22	S	29.89	58	0	poſ.	—	Juſt ſufficient to indicate the kind.
23	S	29.90	59	0	poſ.	—	Serene weather.
24	SE	29.75	60	0	poſ.	—	
25	E	29.68	62	0	—	neg.	Six o'clock A.M. Soon after a fog with drizzling rain, by which the rod became charged poſitively.
26	NE	29.64	61	0	—	neg.	A.M.
27	SW	29.65	60	0	—	neg.	A.M. *Balls* open three-tenths of an inch.
28	SW	29.86	60	0	poſ.	—	But very weak.
29	W	29.72	61	ſtrong	poſ.	—	A very cloudy morning, though at too great a height for my rod; but in the afternoon the clouds approached much nearer, and the rod became charged pretty ſtrongly poſitive, which continued about one hour and a quarter. I charged ſome Leyden bottles with the fluid, ſome poſitive, others negative, for there were four gradual changes of the electricity.
30	S	29.70	56	0	poſ.	—	Balls open half an inch.
31	SW	29.57	58	ſtrong	poſ.	—	Nine o'clock A.M. a heavy ſhower of rain fell, the rod became ſtrongly charged with negative electricity, all the time the ſhower laſted, which was ſhort and ſudden. One hour afterwards, the electricity changed to a ſtrong poſitive, the bell ſuddenly began to ring, and continued to do ſo five minutes; the pith balls then cloſe ſlowly, and open negative, and continued weakly ſo the reſt of the day. At Edinburgh, ſome lightning and thunder this day,
June 1	SW	29.71	53	ſtrong	poſ.	—	A cloudy ſhowery morning. The electric charge in rod was moſt beautiful this day. In about ſix hours time I obſerved ſeven changes of the electricity; five of thoſe changes were gradual. The balls opened from 3 to 4 inches, and remained ſo from 15 to 20 minutes each opening, then gradually cloſed; the other two charged ſlowly, but cloſed very quick.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
June 2	SW	29.85	63	o	poſ.	—	But weak.
3	SW	29.92	62	o	poſ.	—	A.M.
4	W	29.42	52	ſtrong	poſ.	—	A cool cloudy morning. The rod was pretty ſtrongly electrified full ſeven hours to-day, the reſt of the day but weakly ſo. There were 12 changes of the electricity, all gradual, except one, which was inſtantaneous; the balls ſometimes exhibited a waving motion, and ſometimes ſudden jerks. There were ſeveral ſhowers of rain, and two of hail; during the fall of hail, the rod was moſt powerfully electrified, the bell rang very ſtrongly; the effects and appearances were ſo awful, that I kept at a good diſtance from the rod. A great deal of lightning on the eaſt ſide of Kent and Eſſex this day.
5	NW	29.52	53	o	—	neg.	Balls open half an inch.
6	W	29.78	53	ſmall	—	neg.	A.M. and P.M. poſitive; there were ſmall ſparks emitted from the braſs ball L, and the pith balls continued cloſing and opening many times, without any change of kind, for full two hours.
7	NW	29.92	52	o	—	neg.	Balls open from one to ſeven-tenths of an inch.
8	NW	30.18	50	o	—	neg.	
9	NW	30.12	57	o	—	neg.	
10	NW	30.25	56	o	—	—	Weak ſigns, balls not open.
11	NW	30.12	55	o	—	—	
12	E	30.25	56	o	poſ.	—	Balls open three-tenths of an inch.
13	E	30.28	54	o	poſ.	—	Balls open five-tenths of an inch.
14	NE	30.19	51	o	—	neg.	Very cloudy weather, but at too great a height to affect the rod.
15	E	30. 4	55	o	—	neg.	Balls open full half an inch.
16	E	29.90	60	o	—	neg.	Balls open near one inch.
17	S	29.81	62	ſtrong	poſ.	—	Ten o'clock A.M. a ſudden ſhower of rain fell, by which the rod became highly electrified poſitively, and continued to emit ſmall ſparks at the ball L, long after the ſhower was over, without any change of the electricity.

Days.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
June 18	SE	29.89	60	ſmall	poſ.	—	The weather ſhowery; the rod pretty well electrified.
19	SW	29.88	56	o	—	neg.	Balls open ſix-tenths of an inch.
20	SW	29.65	63	o	—	—	Weak ſigns; balls not open.
21	SW	29.71	59	ſmall	—	neg.	This morning ſeveral heavy clouds paſſed over, coming from the SW, by which the rod became moderately electrified negatively. Some rain fell, which increaſed the electrical charge. But, in the afternoon, the wind and clouds put on a more ominous appearance; near five o'clock, began a ſtorm of wind, rain, lightning, and thunder; but the main weight of the ſtorm did not come near my rod. Its dire effects muſt be eaſt of London: nevertheleſs, I had thoſe uſual beautiful appearances which attend a ſtrong charge in the rod. The ſtorm laſted one full hour, during which time there were five ſucceſſive changes of the electricity, *viz.* four gradual ones, attended with ſudden jerks or ſtarts, which often diminiſhed the divergency of the pith balls from 4 inches to 2 inches or leſs. After thoſe jerks the pith balls recovered their former degree of divergency, ſometimes ſuddenly, and at other times ſlowly. The other change happened inſtantaneouſly, the pith balls collapſing and opening ſo quickly, that the eye could barely ſee their motion. There were two other inſtantaneous cloſings and openings of the balls, without a change of the electricity. Much lightning at Graveſend to-day.
22	SW	29.49	56	ſmall	poſ.	—	The rod was in charge all day.
23	SW	29.54	57	ſmall	poſ.	—	Moſt of the forenoon. Afternoon ſome rain fell, and the rod became electrified negatively, and ended poſitively.
24	SW	29.60	61	o	—	neg.	A.M.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
June 25	NW	29.64	55	ſtrong	—	neg.	A very thick cloudy morning. The electric charge in the rod has been moderately ſtrong and very fine to-day. There were five gradual changes of the electricity.
26	W	29.80	53	o	—	neg.	A.M.
27	W	29.96	52	ſtrong	poſ.	—	Cold ſhowery weather. Except ſome ſmall intervals, the rod has been ſtrongly electrified all this day. The bell once rang briſkly for a few minutes, then ſuddenly ſtopped its ringing; the pith balls cloſed and opened negatively, and continued ſo till a little after eight o'clock P.M. when they changed to poſitive. At Liverpool, this day, a tremendous ſtorm of lightning, thunder, hail, and rain.
28	W	29.70	50	ſtrong	—	neg.	A very cold morning and ſhowery, the drops of rain were very large. The rod has been very powerfully electrified near twelve hours this day, during which time there happened eleven changes of the electricity, all gradual but one. The balls often exhibited a waving pendulous motion, without any diminution in their divergency. There were alſo a few of thoſe jerks before noticed on the 21ſt inſtant.
29	W	29.90	51	ſtrong	—	neg.	The bell rang briſkly. There was no change of the electricity.
30	NW	30.10	53	o	poſ.	—	Nearly all day. Weather ſerene and clear.
July 1	N	29.19	76	o	—	—	Weak ſigns, balls not open. An even dark ſky, but fair.
2	N	30.3	76	o	—	—	
3	N	30.2	79	o	poſ.	—	Weather ſerene, very hot, and a clear ſky.
4	N	30.2	80	ſmall	poſ.	—	A.M. and P.M. negative. At Glaſgow, ſome lightning and thunder.
5	NE	29.19	74	o	—	—	Weak ſigns. Balls not open.
6	NE	29.17	72	ſmall	—	neg.	A heavy dark aſmoſphere, but fair. At Monkſilver, near Bath, much lightning and thunder.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	e g.	
		Inches.	°				
July 7	NE	29.16	69	0	—	—	Weak ſigns of electricity; balls not open.
8	NE	30.	60	0	—	—	
9	NE	29.40	72	0	—	—	
10	NE	29.20	72	0	—	neg.	A.M.
11	E	29.15	74	0	—	neg.	A.M.
12	E	29.13	78	0	—	—	Balls not open.
13	W	29. 5	72	ſmall	—	neg.	A.M. and P.M. poſitive with ſparks. At Hereford, this day, a ſtorm of rain, lightning, and thunder.
14	SW	29. 6	69	0	—	—	Weak ſigns; balls not open.
15	NW	28. 8	52	ſmall	—	neg.	A.M. and P.M. poſitive, with bright ſparks. At Strichen, in Scotland, a heavy thunder ſtorm.
16	W	30.	60	0	—	—	Balls not open. At Edinburgh, this day, ſome lightning and thunder.
17	SW	29.10	65	ſmall	—	neg.	P.M. fine ſparks; no change of kind. At Glaſgow and Hamilton, this day, lightning, thunder, hail and rain; and alſo at Newcaſtle, in Northumberland, the ſtorm was ſevere.
18	SW	29.15	59	0	—	—	Weak ſigns; balls not open.
19	W	29.10	50	0	—	neg.	P.M.
20	NW	29.10	54	ſtrong	—	neg.	Three o'clock P.M. I ſaw a thunder ſtorm approaching. While the ſtorm remained at a conſiderable diſtance, the rod was very highly charged with negative electricity, and continued ſo three quarters of an hour; during which time diſtant thunder was heard. Sometimes the balls were affected with a jerking, at others a waving, motion. The wind now ſhifted to the SE, and a heavy rain ſoon came on; the electricity now changed to poſitive, and the bell now rings briſkly; every appearance (both within and without the room) was tremendouſly awful. I therefore ſeated myſelf upon a large inſulated ſtool, where I could with ſafety obſerve the apparatus. A Leyden bottle was undeſignedly placed with its braſs knob near to the braſs

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				ball L. This bottle charged and ſpontaneouſly diſcharged almoſt as quick as I could notice them; and at the ſame time there was a continual flaſhing of denſe ſparks between the bell and braſs ball L. Theſe very grand appearances only laſted ten minutes, and the ſcene was terminated by a clap of thunder; but the ſtorm, and its effects on the rod, laſted near two hours. I ſaw no lightning but what was in the apparatus. There were nine gradual changes of the electricity, from negative to poſitive, &c. I had purpoſely placed a large glaſs bowl, upon an inſulated table, in the open air, to catch the falling electrified rain. As ſoon as the bottom of the bowl was well covered with the rain water of the above ſtorm, to my great ſatiſfaction, a pair of linen threads I had placed for the purpoſe diverged near two inches; the water remained electrified near ten minutes after it was taken into the houſe.
July 21	SW	29.15	52	ſtrong	—	neg	P.M. a little rain fell, and the electricity changed to poſitive. At Edinburgh and Bamff, this day, there was much lightning and thunder.
22	SW	29.10	61	ſtrong	—	neg.	A very ſtrong electricity in the rod all the forenoon. 12 o'clock ſome rain fell, on which the rod became highly charged poſitively, the bell rang weakly for a long time, it then ſtopped, and the balls cloſed, and opened negative, and continued ſo full three hours. This day, near Shrewſbury, a ſtorm of rain, lightning, and thunder.
23	W	29.10	51	ſtrong	poſ.	—	Both A.M. and P.M. and ended negative as the preceding day.
24	W	29.15	71	ſmall	—	neg.	A.M. and P.M. poſitive.
25	W	29.20	71	0	—	—	Weak ſigns; balls not open. An univerſal cloudy dark ſky.
26	W	29.10	69	0	—	—	

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
July 27	N	29.15	64	ſtrong	—	neg.	Four o'clock P M happened a ſudden and ſhort ſtorm of wind and rain. The rod became powerfully electrified, the bell rang for 20 minutes, then ſtopped, and the electricity became poſitive, and ſtrong. At Cambridge, this day, a tremendous ſtorm of lightning and thunder.
28	N	29.20	74	o	poſ.	—	A.M. balls open five-tenths of an inch.
29	W	30.	56	ſmall	—	neg.	A.M.
30	NW	29 30	73	o	—	neg.	Balls open ſix-tenths of an inch.
31	W	29.20	75	o	—	neg.	P.M. a regular dark ſky, with ſmall rain, which laſted four hours; ſuch rain is never electrified ſtrongly. Air is very moiſt.
Aug. 1	W	29.79	60	o	—	—	A heavy dark atmoſphere, and a warm ſoft air. The electricity theſe ſix days amounts only to weak ſigns, balls not open.
2	W	30.12	56	o	—	—	
3	SE	30.	62	o	—	—	
4	E	30. 5	61	o	—	—	
5	E	29.79	66	o	—	—	
6	SW	30. 9	60	o	—	—	
7	W	30 27	56	o	—	neg.	P.M.
8	W	30.24	58	o	—	neg.	A.M.
9	E	30.20	66	o	—	neg.	A.M.
10	E	30 17	60	o	—	neg.	A.M.
11	NE	30.20	66	o	—	—	Balls not open.
12	E	30.16	58	ſtrong	poſ.	—	P.M. a fine ſhower of rain, on which the rod became pretty well electrified; but ſoon changed to a much more ſtrong negative electricity, which afforded fine ſparks at the braſs ball L. The pith balls many times cloſed and opened, without any more changes of kind.
13	E	30.14	60	ſmall	poſ.	—	A.M.
14	NE	30.13	62	o	poſ.	—	A.M. balls open ſeven-tenths of an inch.
15	NE	30. 9	59	ſtrong	poſ.	—	A.M. P.M. a very black cloud paſſed over the rod, by which it became very ſtrongly electrified for a few minutes only; the bell rang briſkly.
16	N	30.15	62	ſtrong	poſ.	—	There has been much rain to-day, and the drops very large. The rod has been

Days.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				in high charge great part of the day. The ſparks at the braſs ball L were very pungent; even the air in the room, and the uninſulated bell, and other things, ſhewed that they were electrified. The bell rang briſkly at three very different times. This ſtrong charge of the rod continued full four hours; and the electricity, during that time, changed in kind ten times. Eight of them were gradual, two of them were quick, and attended with jerks. I heard ſome rumbling of thunder at a great diſtance. At Dunwich, in Suffolk, this day, much lightning and thunder.
Aug. 17	NE	30.26	61	ſmall	—	neg.	A.M.
18	NE	30.32	61	0	—	neg.	Balls open nine-tenths of an inch.
19	NE	30.25	60	0	—	—	} Weak ſigns; balls not open.
20	E	29.95	63	0	—	—	
21	SE	29.75	63	ſtrong	—	neg.	P.M. a ſtrong charge in the rod, but of ſhort duration.
22	W	29.75	56	ſtrong	—	neg.	The rod was in high charge ten hours this day, except a few ſmall intervals; there were nine gradual changes of the electricity, from negative to poſitive, and the contrary. The bell rang very briſkly at three different times during that period. There were ſeveral ſhowers of rain, and one of hail; during the latter, the electric charge in the rod was moſt intenſe; the ſparks darted between the ball and bell extremely ſharp and quick. I found the moiſture in the air of the room was now electrified, alſo the bell, and its metallic connexion with the earth, and even the bricks in the wall to which the metal is faſtened were all electrified with an electricity contrary to that in the inſulated part of the apparatus. An electrician (who had often in vain called at my houſe to ſee

Days.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				the apparatus in high charge) placed his left hand on the bell, and with his finger of the right hand approached the ball L, a denſe ſpark iſſued to it, and he received a ſmart ſhock in his arms and breaſt, like that of the Leyden bottle. We then joined hands, and made the circuit in the uſual way, between the bell and ball, and we both received a ſevere ſhock. Much lightning and thunder this day at Stirling and Dumfries, in Scotland.
Aug. 23	W	29.94	52	ſtrong	poſ.	—	A.M. and P.M. negative on a fall of rain.
24	NW	30.15	58	ſmall	poſ.	—	Great part of the day.
25	SW	30.19	60	0	poſ.	—	A.M. but very weak in the afternoon.
26	SW	30.10	58	0	—	—	} Weak ſigns; balls not open.
27	N	30. 8	59	0	—	—	
28	W	30. 3	54	0	poſ.	—	A.M. a ſerene clear ſky.
29	SE	29.90	58	0	—	—	} Only weak ſigns.
30	SW	29.80	60	0	—	—	
31	NW	29.75	62	0	—	neg.	P.M. from a ſhower of rain, the drops of which were very large. The electricity ſoon changed to poſitive. Though every circumſtance was favourable for a ſtrong electrification, yet the charge in the rod was but weak. My ſuſpicion led me to try the ſtate of the uppermoſt end of the rod, and I found it to be in a contrary ſtate of electricity to that at the lower end of it; the middle part of the rod was in its natural ſtate, that is, ſhewed no ſigns of being electrified; therefore the rod was only (at this time) influentially electrified.
Sept. 1	SE	29.72	58	0	—	neg.	A.M.
2	S	29.60	66	ſmall	poſ.	—	A.M. P.M. a ſhower of rain; the electricity became negative.
3	S	29.50	69	ſmall	poſ.	—	In the forenoon. P.M. a ſtrong gale of wind. I now ſaw thunder clouds forming at a great height. Soon after

Sept.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				five o'clock appearances were dreadful, in five minutes time the ſtrong gale of wind became a ſtorm from SE. This ſtorm of wind (for there was but little rain) carried the huge black clouds to the NW, for there I ſaw abundance of red lightning a little above the horizon, and I once heard a rumbling of thunder. The rod before the ſtorm was poſitive, but by it was changed to negative, and continued ſo during the whole time. The electricity of the rod often varied in ſtrength, ſometimes weak, ſometimes ſtrong, without change of kind. At Amerſham, in Berkſhire, and at the ſame time at the Earl of Aylesford's park, near Packington, in Warwickſhire, was a moſt tremendous ſtorm of hail, rain, lightning, and thunder, from 4 to 6 o'clock P.M.
Sept. 4	SE	29.50	68	ſmall	—	neg.	P.M There were four gradual changes of the electricity in two hours.
5	SW	30.	69	ſmall	—	neg.	P.M. There has been a little rain today.
6	SW	29.95	59	ſmall	poſ.	—	P.M. and near ſun-ſetting negative; there was ſome ſmall rain.
7	SW	30. 3	62	o	poſ.	—	A.M. by the night bottle; that in the rod very weak.
8	SW	30.15	67	o	—	—	Only weak ſigns of electricity; balls not open.
9	SW	30.15	68	o	—	—	
10	SW	30.12	68	o	—	—	
11	N	29.95	61	o	—	—	
12	N	30.10	54	o	poſ.	—	By the night bottle.
13	W	30.10	54	o	—	—	Weak ſigns; balls not open. A dark atmoſphere.
14	SW	29.85	60	o	—	—	
15	W	29.82	48	o	—	—	
16	SW	29.74	48	ſtrong	—	neg.	A.M. There were ſeveral heavy clouds paſſed from the SW, by which the rod was highly charged with poſitive electricity. The bell rang briſkly. This charge laſted from a little before eleven o'clock

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				A.M. to full ſix o'clock P.M. during which time there were four gradual changes of the electricity.
Sept. 17	NW	29.92	44	o	—	neg.	By the night bottle.
18	NW	29.61	52	o	—	neg.	A.M. a more tranſient viſit of the electric fluid I never before ſaw, juſt while a ſmall black cloud paſſed over the rod, and let fall a few drops of rain; the whole time of the charge was about 4 minutes.
19	W	29.40	48	o	—	neg.	A.M. by the night bottle. The electrification of the rod very weak.
20	W	29.40	60	o	—	—	Weak ſigns; balls not open.
21	W	29.40	60	ſmall	—	neg.	A.M. by the night bottle. P.M. the rod was electrified negatively.
22	NW	30. 9	51	o	—	neg.	By the night bottle, and the rod alſo.
23	NW	30. 2	51	o	—	neg.	The electrical charge has been very weak theſe eight days, only juſt ſufficient to indicate the kind.
24	NW	30.23	61	o	—	neg.	
25	NW	30.26	49	o	—	neg.	
26	S	30.26	49	o	—	neg.	
27	SW	30.	61	o	—	neg.	
28	W	30.	51	o	—	neg.	
29	S	29.71	58	o	—	neg.	
30	S	29. 1	48	o	—	neg.	
Oct. 1	SW	29.22	53	ſmall	poſ.	—	A.M. by the night bottle, which was ſufficiently charged to give a bright ſpark on making the circuit. At noon ſome rain fell, by which the rod became electrified negatively, and very ſtrong, which laſted full four hours; during that time, the electricity changed four times, which were all gradual.
2	SW	29.28	47	ſtrong	—	neg.	A.M. P.M. the rod was electrified poſitively, and afterwards negatively, with ſtrong ſparks at the braſs ball L.
3	SW	29.11	48	ſtrong	poſ.	—	A.M. but much more ſtrongly poſitive in the afternoon; the bell for a ſhort time rang briſkly, after that faintly, then ſtopped, and the electricity changed to negative, which continued ſome hours, then declined gradually to weak ſigns only.

Oct.

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
Oct. 4	W	29.40	51	o	poſ.	—	A.M. by the night bottle.
5	S	29.53	45	ſmall	poſ.	—	A M. And P.M. negative.
6	S	29.50	56	ſmall	poſ.	—	A.M. P.M. the ſame, but much ſtronger. There hath been to-day a cold ſmall rain. The electricity changed four times in two hours. This day, at Whitehaven and Lancaſter, was much lightning and thunder, rain, hail, &c.
7	W	29.27	45	ſmall	poſ.	—	A M.; but in the afternoon a much ſtronger poſitive charge.
8	E	29.20	48	o	poſ.	—	A.M.
9	SW	29.29	46	o	poſ.	—	A.M.
10	SE	29.50	45	o	poſ.	—	A.M.
11	SE	29.80	49	o	poſ.	—	A.M.
12	S	29.46	53	o	—	—	Weak ſigns; balls not open. Dark, hazy weather.
13	N	29.50	53	o	—	—	
14	W	29.50	53	o	—	—	
15	W	29.52	56	o	—	—	
16	W	29.67	42	ſmall	poſ.	—	A.M. juſt ſufficient to emit viſible ſparks.
17	W	29.90	40	o	poſ.	—	P.M. by means of a fog.
18	S	29.67	52	o	—	neg.	P.M. balls open ſix-tenths of an inch.
19	S	29.79	50	o	—	—	Weak ſigns; balls not open.
20	SW	29.75	58	o	—	—	
21	S	29.89	53	o	poſ.	—	A.M.
22	W	30. 4	50	o	—	—	Weak ſigns of electricity; balls not open. Dark, hazy weather, and moiſt air.
23	NE	30. 7	53	o	—	—	
24	E	30.24	48	o	—	—	
25	NE	30.28	55	o	—	—	
26	E	30.27	47	o	—	—	
27	E	30.27	45	o	—	—	
28	E	30.30	40	o	—	—	
29	N	30.13	46	o	—	—	
30	E	30. 3	40	o	poſ.	—	P.M. by means of a little cold rain.
31	N	29.92	40	o	poſ.	—	P.M. a very high north wind.
Nov. 1	N	30.32	38	o	—	—	Only weak ſigns of the electric fluid; balls not open. A dark, clouded atmoſphere.
2	N	29.59	46	o	—	—	
3	S	29.19	39	o	—	—	
4	SE	29. 3	48	o	—	—	
5	S	29.29	50	o	—	—	
6	W	28.82	48	o	—	—	

Days.	Wind.	Barom.	Ther	Sparks.	Poſ.	Neg.	
		Inches.	°				
Nov. 7	W	28.90	43	ſmall	—	neg.	A.M. by a ſhower of ſleet, which for a ſhort time occaſioned the rod to emit bright ſparks at the ball L.
8	NW	29. 9	40	o	—	neg.	A.M. by the night bottle.
9	W	29.29	30	o	—	neg.	
10	W	29 90	37	ſmall	poſ.	—	A.M. by means of a fog.
11	W	29 93	39	ſmall	—	neg.	A.M.
12	SW	29.70	45	o	poſ.	—	P.M. This was obtained in the following manner. Soon after ſunſet I perceived a light-coloured dewy vapour ariſe 20 or 30 inches above the ground in the park; the evening being ſerene and fair, I ſtood upon an inſulated ſtool, and waved my exploring rod among the dew [*], and with my finger touched a ſenſible electrometer, which inſtantly opened with poſitive electricity. As the evening advanced, a ſtrong fog filled the air; when it was of ſufficient height for the high or fixed rod, this alſo became electrified with the ſame kind of electricity which I had received near the earth's ſurface.
13	W	29.73	40	o	—	—	Weak ſigns only; balls not open.
14	SW	29.63	46	o	—	—	
15	SW	29.42	47	o	—	—	Notwithſtanding all my care and attention to my pointed rod, this day it has been intirely fruſtrated; for I have not perceived any ſigns of the electric fluid. However, it is the firſt day it has wholly failed me. A moiſt air

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[*] When I find that the moiſture in the air has ſo far injured the inſulation of my high-pointed rod, that it will not retain the electric fluid; in that caſe, I make uſe of a ſmall rod which I hold in my hand, and project through an upper window; having firſt *warmed* the ſtool legs, I place myſelf upon it, &c. I find this method to be a good ſubſtitute in damp weather. The rod is about the length and ſtrength of a fiſhing-rod, with plenty of ſmall wire twined round it.

Days.	Wind.	Barom.	Ther.	Sparks	Poſ.	Neg.	
		Inches.	°				has prevailed for many days; and there have been ſix hours drizzling rain to-day, which muſt leſſen the exactneſs of the inſulation of the rod, and air alſo.
Nov. 16	SW	29.43	43	o	—	—	Weak ſigns; balls not open.
17	S	29.54	47	ſmall	—	neg.	A.M. ſmall ſparks were in the rod, the greateſt part of this day.
18	S	29.51	40	o	poſ.	—	A.M. by means of a fog.
19	S	29.69	43	o	poſ.	—	
20	W	29.65	47	o	poſ.	—	
21	NW	29.86	44	o	—	neg.	A.M. by the night bottle, which I found well charged.
22	N	29.89	44	o	—	—	Weak ſigns only.
23	NE	30.12	42	o	—	—	No ſigns. This is the ſecond failure. A moiſt atmoſphere.
24	NE	30.29	41	ſmall	poſ.	—	A fog. There were viſible ſparks during the greateſt part of the day.
25	N	30.20	37	o	poſ.	—	A.M. by means of a fog.
26	N	30.30	37	ſmall	poſ.	—	A.M. and P.M. poſitive, with fine ſparks.
27	SE	30.45	30	o	—	neg.	At break of day. Afterwards poſitive, by means of a fog.
28	W	30.43	33	o	poſ.	—	All day, by a continued fog.
29	NW	30.30	35	ſmall	poſ.	—	All day. The fog ſtill continues. I have obſerved, during theſe three days and three nights (abating a little time for ſleep, and which I curtailed for ſo noble a purpoſe), that a foggy vapour was conſtantly electrified poſitively. The pith balls diverged from a quarter to three quarters of an inch, except when the fog (which was generally moderate) ſuddenly became thick and dark; then the balls would open to near two inches; at thoſe times I received the electric fluid into bottles. This fog began ſtrongly negative.
30	S	30.10	38	o	poſ.	—	Nearly all day. The fog is entirely gone.
Dec. 1	SE	29.71	38	o	—	—	Weak ſigns; balls not open.
2	S	29.75	50	o	poſ.	—	P.M. A moiſt air to-day.
3	S	29.90	39	ſtrong	poſ.	—	All day (I mean 16 hours out of 24), the weather foggy.

Days.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
Dec. 4	S	30.10	43	o	—	—	Weak ſigns; balls not open.
5	SW	30.38	52	o	—	—	
6	SW	30.40	48	o	—	—	No ſigns of electricity. The third failure. A very moiſt air.
7	SW	30.49	42	o	poſ.	—	A.M. a ſlight fog.
8	SW	30.54	40	ſmall	poſ.	—	P.M.
9	SW	30.55	34	o	poſ.	—	A.M.
10	W	30.52	39	o	—	—	Weak ſigns; balls not open.
11	SW	30.54	42	o	poſ.	—	A.M. and P.M. The weather ſerene and fair.
12	SW	30.41	44	o	poſ.	—	
13	SW	30.30	47	o	—	—	Weak ſigns.
14	S	29.92	44	o	—	neg.	A.M. from a little rain.
15	SW	28.95	45	o	—	—	No ſigns of electricity. The fourth day's failure.
16	SW	29.40	40	ſtrong	poſ.	—	P.M. a ſhort ſhower of ſnow, by which the rod was ſtrongly electrified.
17	W	29.15	38	o	—	—	No ſigns of electricity. The fifth day's failure.
18	W	30.	50	ſmall	poſ.	—	A.M. a fine ſerene morning.
19	SW	29.58	44	o	—	neg.	P.M. by a ſmall rain. On an increaſe of the fall of rain, the electricity changed to poſitive.
20	SW	29.72	44	o	—	—	Weak ſigns.
21	SW	29.78	45	o	—	—	No ſigns of electricity. The ſixth day's failure. A very damp air.
22	SW	29.60	56	o	—	—	No ſigns. The ſeventh day's failure. A moiſt air.
23	W	29.78	49	o	—	—	Weak ſigns of electricity.
24	W	29.27	52	ſmall	—	neg.	At eight o'clock, A.M. began a ſmall ſhower of rain, which was weakly electrified negatively; as the fall of rain increaſed, ſo did the intenſity of the electric fluid increaſe with it, and after continuing for two hours, they both diſappeared together.
25	SW	29.41	42	ſtrong	—	neg.	A.M. by a ſhower of rain. Ten o'clock P.M. a fog ſtrongly electrified poſitive.
26	W	29.89	46	ſmall	poſ.	—	A.M.
27	SW	29.80	46	o	—	—	Weak ſigns.
28	W	29.88	47	o	poſ.	—	P.M. from a very thin fog.
29	SW	29.75	54	o	—	—	Only weak ſigns of electricity.
30	SW	29.76	42	o	—	—	
31	S	29.73	48	o	—	—	

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
1790		Inches.	°				
Jan. 1	SW	29.90	35	o	poſ.	—	Both A.M. and P.M. poſitive.
2	S	30.30	36	ſmall	poſ.	—	All day.
3	S	29.98	46	o	poſ.	—	All the day, by a fog.
4	W	30.20	43	ſmall	poſ.	—	All the day, and bright ſparks ſeveral times to-day.
5	N	30.25	45	ſmall	poſ.	—	All the day, with fine ſparks.
6	SE	30.19	41	o	poſ.	—	A.M.
7	W	30.40	41	ſmall	poſ.	—	A.M. weather dry and foggy.
8	W	30.45	36	ſmall	poſ.	—	Nearly all day in charge.
9	E	30.28	36	o	poſ.	—	A.M. balls open near one inch.
10	SE	30.24	39	o	poſ.	—	A.M. and P.M.
11	S	30.11	41	o	poſ.	—	P.M.
12	W	30.20	45	o	—	—	Weak ſigns.
13	SW	30.34	42	o	poſ.	—	A.M.
14	S	29.95	49	ſmall	—	neg.	A.M. a ſmall rain, by which the rod was electrified negatively.
15	NW	29.91	43	o	poſ	—	A.M.
16	SW	29.95	42	ſmall	poſ.	—	A.M. weather very mild and fair.
17	N	30.28	40	o	poſ.	—	A M.
18	NE	30.20	39	o	poſ.	—	A.M. and P.M.
19	E	30.13	33	o	poſ.	—	A.M. and P.M.
20	SE	30.24	32	ſmall	poſ.	—	A.M.
21	E	30.40	31	ſmall	poſ.	—	A.M. and P.M.
22	W	30.38	35	ſmall	poſ.	—	All the day, from a fog. The ſtrength of the electric charge in the rod was much governed by the occaſional intenſity of the fog; for as one abated in ſtrength ſo did the other, and the contrary. At ten o'clock P.M. I received bright ſparks at the ball L.
23	W	30.33	40	ſmall	poſ.	—	Seven o'clock A.M. I found the rod electrified, ſufficiently ſtrong to emit viſible ſparks, and often afterwards the ſame day.
24	W	30.16	46	ſmall	poſ.	—	A.M.
25	NE	30.11	42	ſmall	—	neg.	There was a moderate ſhower of rain this morning, by which the rod was electrified negatively.
26	W	30. 2	40	o	poſ.	—	A.M. balls open from two to ſeven tenths of an inch.
27	W	29.30	42	o	poſ,	—	
28	S	29.50	44	ſmall	poſ.	—	A.M. at noon fell a ſmall rain, which was void of electricity. Near ten

Jan.

Days.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				
							ten o'clock P.M. a ſudden ſhower rain, ſtrongly negative
Jan. 29	W	29.26	40	o	poſ.	—	P M. A delightful clear fine day, b the atmoſpheric electricity was ve weak till night came on.
30	N	29 50	41	o	poſ.	—	A.M. And P.M. negative.
31	S	29.68	46	o	poſ.	—	Balls open from one to nine tent of an inch, weather fair and ſerene.
Feb. 1	N	29.87	40	o	poſ.	—	
2	SW	30.10	40	o	poſ.	—	
3	W	30.30	44	o	poſ.	—	
4	W	30.60	46	o	poſ.	—	
5	SW	30.61	43	o	poſ.	—	
6	W	30.63	44	o	poſ.	—	
7	NW	30.35	39	o	poſ.	—	
8	N	30.22	36	o	poſ.	—	
9	W	30. 5	40	ſmall	poſ.	—	Sparks juſt viſible.
10	NE	30. 7	43	o	poſ.	—	Serene weather ſtill continues.
11	W	30.20	41	o	poſ.	—	
12	W	30.20	45	o	poſ.	—	
13	SW	30.34	42	ſmall	poſ.	—	Nearly all the day, with fine brig ſparks.
14	SW	30.15	44	o	poſ.	—	Balls open from two to ſix tenths an inch.
15	W	30.22	40	o	poſ.	—	
16	S	29.95	44	o	poſ.	—	A.M. A ſmall rain P.M. which did n occaſion any change in the elect fluid.
17	W	30.20	39	o	poſ.	—	Balls open half an inch, weather ſt ſerene.
18	W	30.42	42	o	poſ.	—	
19	S	30.48	45	o	poſ.	—	
20	SE	30.42	40	ſmall	poſ.	—	A.M. A foggy day.
21	SW	30.38	42	o	poſ.	—	Balls open from one to nine tent of an inch, weather ſtill mild.
22	SW	30.43	43	o	poſ.	—	
23	S	29.97	46	o	poſ.	—	
24	SW	30.15	44	o	poſ.	—	
25	S	29 99	51	o	poſ.	—	
26	W	29.88	51	o	poſ.	—	P.M. I have often obſerved that imp tuous winds leſſen the intenſity of a moſpheric electricity in clear weathe which has been verified this day; for n the leaſt ſign of electricity could be ob tained from the rod till after ſunſe at which time the high weſt wind ha

Day.	Winds.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				ing ſubſided, a little low vapour ſprung up, I then received the fluid in great plenty, and of the ſame kind that it has continued to be for twenty-ſeven days paſt.
Feb. 27	W	30.20	42	o	—	—	Weak ſigns: balls not open.
28	NW	30.22	45	o	—	—	
Mar. 1	N	30.32	47	o	poſ.	—	A.M.
2	NW	30.49	50	ſmall	poſ.	—	A.M. ſparks juſt viſible.
3	N	30.33	49	o	poſ.	—	Very mild ſerene weather.
4	NW	30.27	40	o	poſ	—	
5	NE	30.31	43	o	poſ.	—	
6	NW	30.47	40	o	poſ.	—	
7	NW	30.45	42	o	poſ.	—	
8	SW	30.44	42	ſmall	poſ.	—	A.M. viſible ſparks. Still no change of kind.
9	SW	30.20	41	o	poſ.	—	A.M.
10	SW	29.90	46	o	poſ.	—	P.M. A ſtrong gale of wind to-day. The atmoſphere is extremely dry. The ſun appeared bright all day. No atmoſpheric electricity could be obtained till near ten o'clock at night. This day, at Hallifax, fell a ſhower of ſnow, accompanied with one flaſh of lightning and one clap of thunder.
11	SW	30.20	41	o	poſ.	—	A.M. Still moderate weather.
12	S	30.31	51	o	poſ.	—	
13	SW	30.20	41	o	poſ.	—	A.M. Nine o'clock PM. to eleven o'clock there was a moderate ſhower of rain, which was electrified negatively. I have not till this perceived a negative charge ſince January the 28th.
14	NW	30.50	49	o	poſ.	—	A.M. At Thurſo, in Scotland, hail, lightning, and thunder.
15	N	30.57	51	o	poſ.	—	P.M.
16	N	30.60	40	ſmall	poſ.	—	A.M. viſible ſparks.
17	NE	30.60	40	o	poſ.	—	A.M. a very dry atmoſphere.
18	NE	30.50	45	o	poſ.	—	
19	E	30.55	44	o	poſ.	—	
20	E	30.44	43	o	poſ.	—	A.M. there was an hoar froſt upon the graſs this morning.
21	E	30.39	44	o	poſ.	—	A.M.

Days.	Wind.	Barom.	Ther	Sparks.	Poſ.	Neg.	
		Inches.	°				
Mar. 22	E	30.15	42	o	poſ.	—	A.M. divergency of the pith balls from one to nine tenths of an inch.
23	SW	29.81	46	o	poſ.	—	
24	S	29.80	51	o	poſ.	—	
25	NW	30.	46	o	poſ.	—	
26	E	30. 4	48	o	poſ.	—	
27	E	30.	46	o	—	neg.	A.M. from a ſmall ſhower of rain. P.M. the fluid was poſitive.
28	NE	29.98	44	o	poſ.	—	P.M.
29	N	30.	48	o	poſ.	—	P.M. the air a little foggy.
30	E	29.95	44	o	poſ.	—	A.M. not a beam of ſun has appeared this day.
31	E	29.93	47	o	poſ.	—	A.M. balls half an inch open.
April 1	E	30. 5	45	o	poſ.	—	A.M.
2	E	30.20	41	ſmall	poſ.	—	A.M. viſible ſparks.
3	E	30.25	40	o	poſ.	—	Balls open from one to ſeven tenths of an inch.
4	E	30.19	40	o	poſ.	—	
5	NE	30.23	47	o	poſ.	—	
6	E	29.95	36	o	poſ.	—	
7	E	29.75	45	o	—	—	Weak ſigns of electricity. This weak ſtate has not happened ſince the 31ſt of December. There has been for many days a ſtrong eaſt dry wind, which ſeems hitherto nearly void of the electric fluid.
8	E	29.75	52	o	—	—	
9	E	29.57	66	ſmall	—	neg.	P.M. from a fine ſhower of rain.
10	E	29.50	40	ſmall	—	neg.	A.M. the rain continues, ſo does its negative electricity.
11	E	29.36	35	ſmall	poſ.	—	A.M. from a little fall of ſnow. P.M. ſome ſnow mixed with rain, on which the rod became charged much more ſtrongly poſitive. The rod has been charged full four hours to-day.
12	E	29.35	39	o	—	—	Weak ſigns; balls not open.
13	E	29.70	38	ſmall	—	neg.	A.M. a moderate rain, but ſtrongly electrified, and continued ſo full two hours. There were two gradual changes of electricity.
14	NE	29.81	41	o	—	neg.	P.M. but after ſunſet the rod was electrified poſitively.
15	E	29.68	35	ſmall	—	neg.	Six o'clock A.M. a little rain fell. Half after eight o'clock, a fine ſhower of

Days.	Wind.	Barom.	Ther.	Sparks.	Poſ.	Neg.	
		Inches.	°				ſnow; the rod now became ſtrongly electrified poſitively. Denſe ſparks were now received at the ball L; half after nine o'clock, the electricity changed to negative. I caught ſome of the ſnow in the apparatus mentioned the 20th of July in this journal, and I found it weakly electrified.
April 16	NE	29.85	41	o	poſ.	—	
17	N	29.98	42	o	poſ.	—	Divergency of the pith balls was from a tenth to ſeven-tenths of an inch. Fine ſerene weather.
18	NE	29 80	40	o	poſ.	—	
19	NE	29.69	41	o	poſ.	—	
20	SE	30.26	45	o	poſ.	—	
21	S	30.23	47	o	poſ.	—	
22	S	29.94	51	o	poſ.	—	
23	SW	29.70	55	o	poſ.	—	
24	W	29.55	52	o	poſ.	—	A M. and P.M. negative from a little fall of rain.
25	SW	29.60	51	ſtrong	poſ.	—	Half paſt nine o'clock A.M. A diſtinct black cloud approached the rod, and ſome heavy drops of rain fell; the electricity of the rod then changed to negative. About half paſt three o'clock P.M. a very large low cloud paſſed over the rod, and rained a little, on which the rod became ſtrongly electrified poſitively.
26	NE	29.85	53	ſmall	poſ.	—	A.M. P.M. on the fall of ſome rain, the rod was charged negatively.
27	N	29.98	47	o	poſ.	—	The divergency of the balls from two to ſix tenths of an inch. Serene fine weather.
28	W	29.80	50	o	poſ.	—	
29		29.69	51	o	poſ.	—	
30		29.58	53	o	poſ.	—	
May 1	E	29.75	50	ſmall	poſ.	—	Nine o'clock A.M. a ſhower of rain; the electricity now became negative. The rod has been electrified to-day from ſix o'clock A.M. to ten o'clock P.M. and I ſuppoſe all night alſo.
2	N	30.22	41	o	poſ.	—	Divergency of the pith balls from three to ſeven tenths of an inch.
3	NE	29.85	49	o	poſ.	—	
4	SE	29.63	51	ſmall	poſ.	—	A.M.

Days.	Wind	Barom.	Ther.	Sparks.	Poſ.	Neg	
		Inches.	°				
May 5	S	29.73	52	ſtrong	poſ.	—	Six o'clock, A.M. very cloudy. Eight o'clock it rained, the electricity now became ſtrongly negative, with fine ſparks at the ball L. This ſhower having ceaſed, another ſoon followed, which electrified the rod poſitively. The rod was charged 7 hours to-day.
6	SW	29.75	50	ſmall	—	neg.	Every appearance at the rod to-day was nearly as during the preceding one.
7	W	30.10	56	0	poſ.	—	Balls open from one to five tenths of an inch. Such weak ſigns of electricity, as have been obſerved for theſe two days, are the uſual effects of a very ſtrong and dry weſterly wind; and in general, let a ſtrong dry wind blow from what point of the compaſs it may, it is attended with weak ſigns of electricity.
8	W	29.84	50	0	poſ.	—	

The above-mentioned eighth day of May completes this journal of one whole year, which I give to the curious in atmoſpheric electricity as a faithful narrative of facts, having never once deputed another perſon to make obſervations for me.

JOHN READ,
of Knightſbridge, near London.

A monthly account of electrical ſparks, and of poſitive and negative electricity, as indicated by the pith-ball electrometer, and ſometimes by only flaxen threads without balls to them.

		Times		Times.	Number of days in each month in which ſparks were perceived. Days.
23 days of May, 1789, 8 days of May, 1790,	Poſitive	17	Negative	18	9
June	Poſitive	32	Negative	36	12
July	Poſitive	13	Negative	22	12
Auguſt	Poſitive	19	Negative	19	9
September	Poſitive	9	Negative	23	7
October	Poſitive	17	Negative		7
November	Poſitive	12	Negative		8
December	Poſitive	12	Negative		7
January	Poſitive	26	Negative		13
February	Poſitive	26	Negative		3
March	Poſitive	30	Negative		3
April	Poſitive	28	Negative	12	8
		241		156	98

It appears from this journal, that there were only *ſeven* days throughout the year in which no ſigns of electricity were perceived; *viz.* the 15th and 23d of November, and the 6th, 15th, 17th, 21ſt, and 22d of December.

Remarks on the phænomena exhibited by the rod on the 31ſt of Auguſt.

I was for a long time extremely puzzled to account for the rapid changes which the pith balls on ſome days ſo frequently exhibited; being poſitive one minute, then negative for another, and the next returning again to poſitive. From often conſidering this apparently whimſical changeableneſs in nature, I was at length induced to ſuſpect, what indeed was afterwards confirmed by actual experiment, *viz.* that ſome of theſe changes are only apparent, and not real, they being occaſioned not by the actual communication of a different ſort of electricity, but merely by the action of electrical atmoſpheres; thus, when an electrified cloud comes within a certain diſtance of the rod, and before it comes near enough to impart to it ſome of its own electricity, the electrical atmoſphere of the former, agreeable to the well known laws of electricity, will diſturb the electric fluid naturally belonging to the rod, and will conſequently occaſion ſeveral apparent changes in the electrometer, which changes an unexperienced obſerver would attribute intirely to the change of electricity in the clouds.

This obſervation was evidently confirmed by the phænomena obſerved on the 31ſt of Auguſt; and thence it appears, that the real number of changes from poſitive to negative, or from negative to poſitive electricity, cannot be ſo great as it is ſhewn by the electrometer affixed to the rod.

I cannot help lamenting with Signor BECCARIA, that there are ſo few high pointed rods erected to aſcertain the electrical ſtate of the earth and atmoſphere at all times; but more particularly during thunder ſtorms. If there had been pointed rods, for inſtance, at Whitehaven and Lancaſter on the 6th of October, and well attended to at the time of the ſtorm of lightning and thunder, which happened at both places nearly at the ſame time, it would then have been known, whether the apparatus might not be poſitive at one place when it is negative at the other.

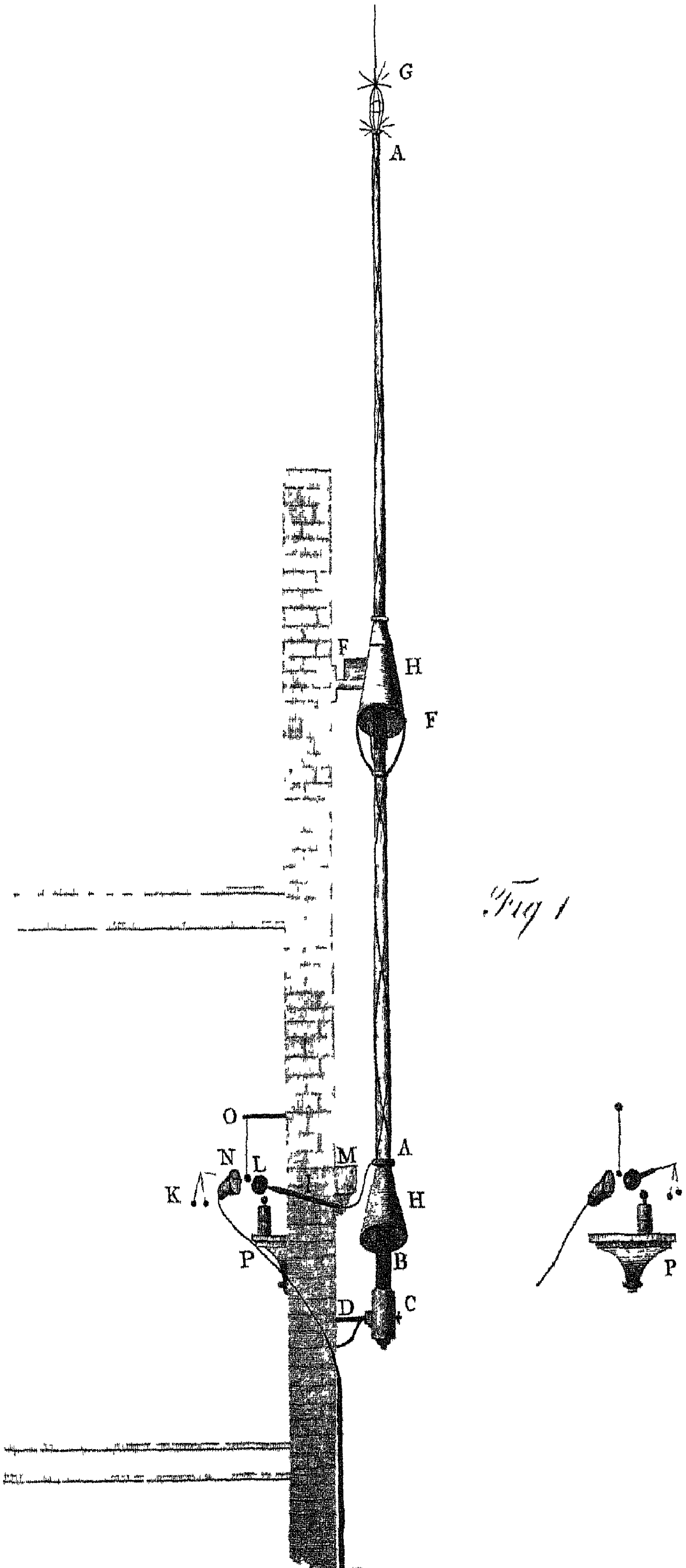

Fig 1

J. Read del.

XIII. *Farther Experiments relating to the Decompoſition of dephlogiſticated and inflammable Air. By* Joſeph Prieſtley, *LL.D. F. R. S.*

Read April 7, 1791.

THE doctrine of *phlogiſton*, and that of the *decompoſition of water*, have long engaged the attention of philoſophical chemiſts, and experiments have ſometimes ſeemed to favour one concluſion, and ſometimes an oppoſite one. I have myſelf been very differently inclined at different times, as appears in my publications on the ſubject; and I am hardly ſenſible of a wiſh which way this important controverſy, as it may be called, be decided, notwithſtanding the part that I have taken in it. I cannot help thinking, however, that the experiments, an account of which I ſhall now lay before the Society, are deciſive in favour of the compoſition of an acid from dephlogiſticated and inflammable air; and, therefore, that the opinion of theſe two kinds of air neceſſarily compoſing *water* cannot be well founded. It is, indeed, ſufficiently evident, that the ſame elements likewiſe compoſe *fixed air*, and therefore it is the leſs extraordinary that they ſhould compoſe another acid.

The doctrine of phlogiſton I would, however, obſerve, will not be affected by the moſt deciſive proof of the compoſition of water from dephlogiſticated and inflammable air; ſince this would only prove, that phlogiſton is one conſtituent part of water; which is an opinion that I have advanced, and mentioned on ſeveral occaſions; and it is the leſs extraordinary, as water reſembles metals in the remarkable property of being a pretty good conductor of electricity. What I ſhall now allege, however, will make it very doubtful, whether pure water be ever formed by the union of dephlogiſticated and inflammable air; and perhaps make it more probable, that water, as I have lately advanced, is only the *baſis* of thoſe kinds of air, as well as of every other kind.

It was objected to my former experiments on the decompoſition of dephlogiſticated and inflammable air, by firing them together in a copper veſſel, which always produced an acid liquor, that this acid came from the *phlogiſticated air* with which the dephlogiſticated air that I made uſe of was neceſſarily more or leſs diluted; or from that which I could not wholly exclude, as a part of atmoſpherical air, when I exhauſted the copper veſſel by means of an air-pump.

To obviate this objection, I then obſerved, that I not only conſtantly found that the more phlogiſticated air was contained in the two other kinds of air (mixed in the proportion of two meaſures of inflammable air to one of dephlogiſticated) the leſs acid I got; but that, when I purpoſely mixed any given quantity of phlogiſticated air with them, it appeared not to have been at all affected by the proceſs, but remained the very ſame, in quantity and quality, as before. Still, however, becauſe Mr. CAVENDISH, though in a very different proceſs, had found nitrous acid to reſult from the decompoſition of phlogiſticated and dephlogiſticated air; and becauſe M. LAVOISIER and his friends had found nothing but pure water after the ſlow burning of dephlogiſticated and inflammable air; it was maintained by the favourers of their ſyſtem, that the *water* only in the liquor which I procured came from the union of the two kinds of air, and the *acid* from the *phlogiſticated air* which I had not been able to exclude.

But let any perſon only conſider the very ſmall quantity of nitrous acid which was procured by Mr. CAVENDISH from the certain decompoſition of 3194 grain meaſures of atmoſpherical air, amounting to more than 6½ ounce meaſures in one caſe, and of 2710 grain meaſures, amounting to 5½ ounce meaſures in another caſe (Phil. Tranſ. Vol. LXXVIII. p. 264. 268.), three-fourths of which was phlogiſticated air; and the vaſtly greater quantity which I procured (Ibid. p. 324.), when it could not be proved, that a particle of phlogiſticated air was decompoſed, and think whether it was at all probable, that the acid came from this kind of air, and not from the union of the dephlogiſticated and inflammable air, which evidently diſappeared in very great quantities. This circumſtance alone might have ſatisfied thoſe who intereſt themſelves in this queſtion; but it does not ſeem to have been attended to.

I have now, however, effectually removed the objection above mentioned, by intirely excluding all phlogiſticated air from the proceſs; the dephlogiſticated air which I at preſent uſe being ſo pure, that it contains no ſenſible quantity of phlogiſticated air. I alſo make uſe of no air-pump, but firſt fill the copper veſſel with water, and then diſplace it by the mixture of the two kinds of air; yet, in theſe circumſtances, in which all phlogiſticated air is excluded, I procure even a ſtronger acid than before.

The paper that I ſend along with this article contains the dry reſiduum of the turbid green liquor, produced by a ſingle exploſion of a mixture of two parts inflammable and ſomething

thing more than one part of dephlogiſticated air, in a copper veſſel which holds thirty-ſeven ounces of water; and a little more muſt have have remained in the veſſel, which I could not get out by draining or ſhaking it. It is moſt evident, therefore, that the acid neceſſary to diſſolve ſo much copper muſt have come from the union of the dephlogiſticated and inflammable air, becauſe there was nothing elſe in the veſſel. The inflammable air was procured from iron by means of ſteam.

This very pure dephlogiſticated air I firſt imagined could only be got by the proceſs in which I obſerved (Experiments on Air, Vol. II. p. 170.) that I once before procured it, though I then ſuppoſed the extraordinary reſult to be accidental; becauſe in other circumſtances I have ſometimes had it very pure when I could not ſucceed in a ſecond attempt of the ſame kind. It was by heating the yellow product of the ſolution of mercury in ſpirit of nitre, without ſuffering the red precipitate into which it is converted by heat to come into contact with the external air, from which I thought it probable that it might attract ſome phlogiſton. Afterwards, however, I found that this circumſtance makes no difference whatever; and that the air ſo procured *appeared* to be purer, aroſe from the greater purity of the *nitrous air* which I made uſe of as a teſt, and which I got from mercury, and not from copper, the nitrous air from which I find to be much leſs pure. For trying the dephlogiſticated air yielded by ſome red precipitate which had been prepared many months by the nitrous air from mercury, it appeared to be as pure as that which was procured in the manner above deſcribed.

That the dephlogiſticated air which I now made uſe of was ſufficiently pure for my purpoſe, appeared from mixing one meaſure of it with two of nitrous air, when the whole quantity was reduced to leſs than four hundredth parts of one meaſure; ſo that it is probable that, by a more accurate proportion of the two kinds of air, and greater addreſs in mixing them, they might have almoſt intirely diſappeared. There is beſides ſome reaſon to think, from the great variety in nitrous air, that the greater part of this very ſmall reſiduum comes from the nitrous air, and not from the dephlogiſticated.

It will be ſaid, how is it poſſible to reconcile the reſult of this experiment with that of M. LAVOISIER and his friends? which I was by no means diſpoſed to queſtion after the publication of the *Extract from the Regiſter of the Academy of Sciences for Auguſt* 28, 1790, in the ſeventh volume of the *Annales de Chimie*, in which a diſtinct account is given of a large quantity of very pure water procured from the ſlow combuſtion of the two kinds of air above mentioned: for before this it was acknowledged, that ſome little acid was always found in the water ſo procured.

But my late experiments, beſides aſcertaining the fact of the production of nitrous acid from the decompoſition of dephlogiſticated and inflammable air, throw ſome farther light on the ſubject, and may in ſome meaſure explain their reſult; for I am now able to procure, in my own proceſs, either nitrous acid or pure water, from the ſame materials.

I conſtantly obſerve, that if there be a ſurplus of dephlogiſticated air, the reſult of the exploſion is always the acid liquor; but that if there be a ſurplus of inflammable air, the reſult is ſimply water. That phlogiſticated air is not in all caſes affected by this proceſs, I completely aſcertained, by admitting a little common air into that mixture of the two kinds of air which always produced water, and finding nothing but water in the reſult.

I find,

I find, however, that, agreeably to the experiments of Mr. CAVENDISH, phlogiſticated air is decompoſed in this proceſs, when there is not enough of inflammable air to ſaturate the dephlogiſticated air; though when there is a redundancy of inflammable air, there is even a production of phlogiſticated air. Putting 0.5 oz. m. of phlogiſticated air to a mixture of two ounce meaſures of inflammable air and 1 5 oz. m. of dephlogiſticated air, the whole was reduced by exploſion to 1.05 oz. m. of the ſtandard of 1.1, with two meaſures of dephlogiſticated air, which appears by computation to contain no more than 0,388 oz. m. of phlogiſticated air; ſo that 0.112 oz. m. had been decompoſed in the proceſs. When there is a ſufficient quantity of inflammable air, the phlogiſticated air always remains unaffected in this proceſs, as appears by mixing any quantity of it with the two kinds of air to be exploded, and finding the very ſame quantity, as I have repeatedly done, in the reſiduum.

That when there was a ſufficiency of inflammable air for the purpoſe, phlogiſticated air is even *produced* in this proceſs, was evident from my never being able to diminiſh any quantity of dephlogiſticated air by inflammable air ſo far as by good nitrous air, and the reſiduum always containing phlogiſticated air. Having exploded two meaſures of inflammable air with one of dephlogiſticated air, which by a mixture of two meaſures of nitrous air was reduced to 0.04, there was a reſiduum of 0.1, of the ſtandard of 1.3, which appears by computation to contain 0.0767 oz. m. of phlogiſticated air.

The reaſon why, in my former experiments, I always procured more or leſs acid, muſt have been that, without any intention, or ſuſpecting that any thing depended upon it, I muſt have had ſome ſurplus of dephlogiſticated air. M. LAVOISIER I alſo perceive to have taken it for granted, as I did, that after either of our proceſſes, any ſurplus of either of the two kinds of air would only have remained unſaturated, and have been found unchanged in the reſiduum.

I claim no merit whatever in this obſervation. It was in conſequence of accidentally finding pure water in what I then imagined to be the ſame circumſtances in which I had always before found acid, and which ſurprized me not a little at the time, that I was led to vary the proportions of the two kinds of air, till at length I ſucceeded in aſcertaining the circumſtances on which this remarkable difference in the reſult depends; but I am by no means able to aſſign any reaſon for this difference.

In this ſtate of my experiments I concluded, that *nitrous acid*, though conſiſting of the ſame elements with pure water, contains a greater proportion of dephlogiſticated air; and in the laſt edition of my *Obſervations on Air*, Vol. III. p. 543. I obſerved, that " ſubſtances, poſſeſſed of very different proper-" ties, may be compoſed of the ſame elements, in different pro-" portions, and different modes of combination. It cannot " therefore be ſaid to be abſolutely impoſſible, but that water " may be compoſed of theſe elements," *viz*. dephlogiſticated and inflammable air.

When I firſt prepared an account of my late experiments for the Royal Society, I entertained this idea; but I now conſider it as at leaſt uncertain, becauſe when I mix the two kinds of air in ſuch proportions as to produce *water*, I find in the reſiduum much more *phlogiſticated air* than I do when *acid* is produced, which affords a ſuſpicion that, in this caſe, *the principle of acidity* goes wholly into the phlogiſticated air, which, as my former experiments ſhew, actually contains it, though it is not eaſy to aſcertain in what proportion.

Having exploded three ounce meaſures of a mixture of ſomething more than two parts inflammable air, and one of dephlogiſticated, and another equal quantity in which the inflammable air bore a leſs proportion to the dephlogiſticated, the former of which I knew would yield water, and the latter acid, I found the reſiduum of the former to be 0.57 oz. m. not affected by nitrous air, and weakly inflammable; and in order to find how much phlogiſticated air it contained, I mixed different proportions of phlogiſticated and inflammable air, and concluded, from the manner of firing *them*, and this *reſiduum*, that it could not conſiſt of leſs than one-third of phlogiſticated air, *viz*. 0.19 oz. m. But the reſiduum of the mixture which would have produced acid was 0.62 oz. m. of the ſtandard of 1.0, which I find by computation to contain not more than 0.062 oz. m. of phlogiſticated air. I repeated this experiment very many times, and never failed to have a ſimilar reſult; ſo that it is very poſſible that the *pure water* we find may be nothing more than the baſis of the two kinds of air; and the principle of acidity in the dephlogiſticated air, and the phlogiſton in the inflammable air, may combine to form a ſuperfluous acid in the one caſe, and the phlogiſticated air in the other.

This ſuppoſition is ſtrengthened by finding that whether the produce be acid, or pure water, the two kinds of air unite in nearly the ſame proportions. But ſince water has an affinity to almoſt every ſubſtance in nature, and a peculiarly ſtrong one to the acid and alkaline principles, it may be impoſſible that it ſhould be wholly free from them; and if they be in proper proportions to ſaturate one another, and in the ſame quantities, their preſence may never appear.

As the reaſon why, in my former experiments, I always produced an acid liquor, and never pure water, was my uſing too great a proportion of dephlogiſticated air; ſo the reaſon why M. Lavoisier and his friends generally produced but little acid, and at laſt not at all, muſt have been, that the *ſlow combuſtion* which they made uſe of gave the principle of acidity in the dephlogiſticated air, and the phlogiſton in the inflammable air, a better opportunity of eſcaping, and forming the phlogiſticated air in their reſiduum, of which they have not publiſhed any ſatisfactory account *; and it is probable, that the weight of theſe elements compared with that of the water which forms the baſis of the two kinds of air, may be very ſmall. That excellent philoſopher M. De Luc ſuppoſes that they have even no weight at all.

M. Lavoisier himſelf, I obſerve, lays particular ſtreſs, (p. 262.) on the *ſlowneſs* of the combuſtion, as if he ſuſpected it to be neceſſary to his reſult. This circumſtance may alſo account for my want of ſucceſs in the attempts that I made to repeat his experiment: for whenever I made a ſtream of inflammable air to burn in a veſſel of dephlogiſticated air (which I contrived to do by means of a leſs expenſive, but I own a leſs accurate, apparatus than his) I always got ſome acid, though leſs than in my own proceſs; but I made a larger and ſtronger flame than I imagine M. Lavoisier choſe to produce.

In the courſe of theſe experiments, I found, that when the inflammable air I made uſe of was from turnings of *caſt iron*, there was always a conſiderable quantity of *fixed air* in the reſiduum, not leſs than one-tenth of a meaſure, after the exploſion of two meaſures of inflammable air and one of dephlogiſticated; whereas there was either no fixed air at all, or the ſlighteſt appearance of it imaginable, when I made uſe of inflammable air from *malleable iron*, extracted either by means of ſteam or acids.

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* Since this was written, Meſſ. Fourcroy, Vauquelin, and Seguin, have publiſhed a very particular account of their experiment; from which it appears, that, after the combuſtion of the two kinds of air, there was a pretty large reſiduum of phlogiſticated air, more than was contained in the airs before combuſtion. See *Annales de Chimie*, for April 1791, p. 35.

The principal of theſe experiments, as well as thoſe in my former Papers on this ſubject, will be found to confirm the ſimilar ones of Mr. CAVENDISH; but they prove the ſource of the *acid* in the reſults not to be what he imagined, *viz. phlogiſticated air*, but the union of the dephlogiſticated and inflammable air; and they alſo make it at leaſt doubtful, whether theſe two kinds of air compoſe *pure water*.

XIV. *Experiments on Human Calculi. In a Letter from Mr.* Timothy Lane, *F. R. S. to* William Pitcairn, *M. D. F. R. S.*

Read May 5, 1791.

SIR,

IT will give me much ſatisfaction ſhould the following experiments, made twenty years ago, ſo far meet your approbation, as to be thought worthy the notice of the Royal Society.

The Lixivium Saponarium of the late Pharmacopœia, prepared with the addition of ſo much lime as nearly to free the ſalt of tartar of its fixed air *, having been uſed as a medicine for the ſtone and gravel ſome years before, and its effects found very unequal, I thought it neceſſary to examine different calculi, then collected, both as to the effect of the above lixivium, and of fire, upon them.

Great diſparity was obſerved; ſome being diſſolved, and others ſcarcely altered in their figure.

When tried by fire, ſome were nearly evaporated by a red heat, and others retained their form.

Different parts even of the ſame calculus, varied conſiderably.

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* See Letter to Dr. HEBERDEN, Medical Tranſactions, Vol. I. p. 112.

That I might be better informed of the above, the experiments were repeated both by fire and lixivium, with greater accuracy, as follows.

Fourteen ſpecimens were ſelected, ſome of which were parts of the ſame calculus, and others different calculi.

In the experiments by fire I was favoured with the aſſiſtance of Mr. STANESBY ALCHORNE, of the Tower, to whom were ſent ten grains of each, in ſeparate papers, which were numbered.

The contents of each paper were placed in ſeparate cupels, under a muffle, the ſame as is uſed by him for aſſaying gold and ſilver. The fire was raiſed gradually, till the furnace was fully heated: the time from raiſing the fire to the taking them out again was three hours, when it was concluded, that whatever volatile matter they contained was expelled.

The ſame quantity as above, of each ſpecimen, being put into ſeparate numbered phials, with one ounce meaſure of the lixivium in each, continued forty-eight hours; the phials were frequently ſhaken to forward the ſolution.

The clear liquor of each phial was decanted into freſh phials, and a quarter of an ounce more lixivium was added to ſuch as were undiſſolved; after twenty-four hours they were poured out of the phials into ſeparate filtering papers, each numbered, and the phials waſhed with diſtilled water, which was alſo poured into the papers, ſo that all that remained undiſſolved might be detained by the papers, which with their contents were carefully dried.

The remains of each

	Unſublimed.	Undiſſolved.
	Grains.	Grains.
Nº 1.	1½	
2.	2½	2
3.	¼	½
4.	1¼	2
5.	¼	0
6.	3½	2½
7.	3¼	6
8.	6	8¼
9.	6¼	6¼
10.	6¼	7½
11.	¼	½
12.	¼	0
13.	5½	4
14.	6	5¾

Appearances of each after Calcination.

Nº 1. 3. 7. 8. left a fine white and ſoft powder.

Nº 4. 5. 11. 12. left a white and gritty powder.

Nº 2. 6. 9. 10. 14. were partly in powder white and gritty, with ſome lumps of a dark colour, as if not fully calcined.

Nº 13. Of this the figure was not greatly altered; it remained hard, and part of it appeared as if inclined to flux.

After being in the lixivium forty-eight hours.

Nº 8. 9. 13. 14. were found ſoft.

Nº 7. and 10. remained hard.

Theſe ſix were ſeparately taken out of the lixivium and put into a mortar, and rubbed or broken, and then carefully returned to their ſeparate phials before the ſecond addition of lixivium, in order to forward the ſolution.

Specimens deſcribed.

N° 1. The external part of a laminated calculus, of a light yellowiſh brown colour *.

N° 2. The external part of a calculus, in colour like dirty tobacco-pipe clay †.

N° 3. A light-brown laminated calculus.

N° 4. and 5. Two ſpecimens from one calculus; of which N° 4. is the external coat, of a dirty tobacco-pipe-clay colour.

N° 5 The internal part of N° 4., yellowiſh like N° 1.

N° 6. A calculus taken out of the urethra; a greyiſh white, inclining to yellow, of a porous texture.

N° 7. A calculus about the ſize of a nutmeg, taken from a child of a year old, given me by the late Mr. POTT; aſh-coloured, in waves of different ſhades, laminated and hard.

N° 8. A dark-brown very hard calculus, of the mulberry kind.

N° 9. and 10. Two ſpecimens from one calculus; of which N° 9. is the external whitiſh part, which appeared like a coat of calcareous earth, covering an irregular mulberry calculus ‡.

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* The nucleus, ſo called, being the central part, was of a much deeper colour, and had been found not ſo ſoluble in lixivium as the light-brown part.

† The nucleus was of a bright yellow, and more ſoluble in lixivium than the whitiſh part.

‡ The covering of this calculus induced me to ſuſpect that lime or lime-water might have been taken, and, by being decompounded by freſh urine, containing fixed air, form this covering. Other calculi have afforded the ſame ſuſpicions.

In future, an account of medicines taken might afford much information, joined with the examination of different parts of large calculi taken out of the bladder.

N° 10. The brown mulberry part covered by N° 9. The three following are parts of one large, laminated calculus; of which

N° 11. Is the external lamina, of a browniſh yellow.

N° 12. The central part, called the nucleus, of a pale orange colour.

N° 13. Some of the laminæ, between the nucleus and the external coat, of a ſparkling appearance.

N° 14. A whitiſh, porous, and eaſily broken calculus.

The experiments by fire explain the unequal accounts of authors, reſpecting the component parts of calculi.

In general, thoſe which contain the largeſt proportion of volatile parts were moſt ſoluble in lixivium.

The inſolubility of ſome explains the want of ſucceſs in ſeveral caſes, where lixivium, ſoap, and lime-water, have been given as remedies.

The ſolubility of others, joined with the teſtimony of reputable authors, and my own experience for near thirty years, confirm the ſalutary effects of lixivium in many caſes.

It frequently happens, in fits of the gravel and ſtone, that gravel or ſmall pieces of calculi are diſcharged, which ſhould be examined.

If perfectly ſoluble in lixivium *(Aq. kali puri)*, the remedy is obvious; if imperfectly, doubtful; if inſoluble, lixivium will only irritate, without benefit.

I am, &c.

T. LANE.

Alderſgate-ſtreet,
March 10, 1791.

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XV. *Chermes Lacca.* *By* William Roxburgh, *M. D. of* Samulcotta. *Communicated by* Patrick Ruſſell, *M. D. F. R. S.*

Read May 19, 1791.

SOME pieces of very freſh-looking lac, adhering to ſmall branches of Mimoſa cinerea *, were brought me from the mountains, on the 20th of November, 1789. I kept them carefully in wide-mouthed cryſtal bottles, ſlightly covered; and this day, the 4th of December, fourteen days from the time they came from the hills, thouſands of exceeding minute red animals were obſerved crawling about the lac and the branches it adhered to, and ſtill more were iſſuing from ſmall holes on the ſurface of the cells. By the aſſiſtance of glaſſes, ſmall imperforated excreſcences were alſo obſerved, interſperſed among theſe holes, two, regularly, to each hole, crowned with ſome very fine white hairs, which being rubbed off, two white ſpots appeared. The animals, when ſingle, ran about pretty briſkly; but, in general, on opening the cells, they were ſo numerous as to be crowded over one another. The ſubſtance of which the cells were formed cannot be better deſcribed, with reſpect to appearance, than by ſaying it is like the tranſparent amber that beads are made of. The external covering of the cells may be about half a line thick, is remarkably ſtrong, and able to reſiſt injuries: the partitions are much thinner. The cells are in general irregular ſquares, pentagons, and hexagons, about an eighth of an inch in diameter, and a quarter of an inch deep: they have no communication with each other. All thoſe I opened, during the time the animals were iſſuing from them, contained in one ſide, and which occupied half the cell, a ſmall bag, filled with a thick red jelly-like liquor, replete with what I take to be eggs. Theſe bags, or utriculi, adhere to the bottom of the cells, and have each two necks, which paſs through perforations in the external coat of the cells, forming the before-mentioned excreſcences, ending in ſome very fine hairs.

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* Lac, on this coaſt, is always found upon the three following ſpecies of Mimoſa; 1ſt, a new ſpecies, called by the Gentoos Conda corinda; 2d, Mimoſa glauca of Kœnig; and, 3dly, Mimoſa cinerea of LINNÆUS.

The other half of the cells have a diſtant opening, and contain a white ſubſtance, like ſome few filaments of cotton rolled together, and a number of the little red inſects themſelves crawling about, ready to make their exit. Their portion of each cell is about a half; and, I think, muſt have contained near one hundred of theſe animals. Other cells, leſs forward, contained in this half with one opening, a thick, red, dark blood-coloured liquor, with numbers of exceedingly minute eggs, many times ſmaller than thoſe found in the ſmall bags which occupied the other half of the cells. Several of theſe inſects I obſerved to have drawn up their legs, and to lie flat; they did not move on being touched; nor did they ſhew any ſigns of life upon the greateſt irritation *.

Dec. 5. The ſame minute hexapodes continue iſſuing from their cells in numbers.

Dec. 6. The male inſect, I have found to-day, at leaſt what I think is ſuch. A few of them are conſtantly running about, and over the little red inſects, (which I ſhall now call the female) moſt actively: as yet they are ſcarce, not more, I imagine, than 1 to 5000 females, but they are four or five times their ſize.

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* It will appear in the ſequel, that theſe were on the point of transformation into the pupa ſtate.

To-day the female inſects continue iſſuing in great numbers, and move about as before.

Dec. 7. The ſmall red or female inſects, are ſtill more numerous, and move about as before. The winged or male inſects, are ſtill very few, but continue active. There have been freſh leaves and bits of the branches of Mimoſa cinerea, and Mimoſa intſia, put in to them. They go over them indifferently, without ſhewing any preference or inclination to work, or to copulate. I opened a cell, from whence I thought the winged flies had come, and found ſeveral (eight or ten) ſtruggling to ſhake off their incumbrances. They were in one of thoſe utriculi mentioned before, which end in two mouths, ſhut up with fine white hairs; but one of them was open for the exit of the flies; the other would, no doubt, have opened in due time. This utriculus I found now perfectly dry; and could plainly ſee it was divided into minute cells, by exceedingly thin membraneous partitions. I imagine, before any of the flies made their eſcape, it might have contained about ſixteen or twenty. In the minute cells, with the living flies, or from whence they had made their eſcape, were ſmall dark-coloured compreſſed grains.

March 26, 1790, I found ſome branches of the ſame ſort of Mimoſa, with numbers of the minute red hexapodes, mentioned in December (ſeemingly in their pupa ſtate), adhering to them. They are of various ſizes, from half a line to a line and a half in length. I found many of the large ones empty. They have a round opening at the lower end, with a ſmall round operculum, or lid, which now looſely covers the empty huſk or ſhell: the inſide of theſe is lined with a ſmall white membrane; others were ſtill ſhut, ſome were opening, and ſome half open, with the inſects projecting more or leſs, and ſoon extricating themſelves entirely.

I opened ſome of the middle-ſized, and found they contained a thick, deep blood-coloured liquid; others, ſtill larger, put on the appearance of the fly, which was ſoon to iſſue, retrograde.

Deſcription of the male lac inſect in its perfect ſtate.

It is then about the ſize of a very ſmall fly, and exceedingly active; the larva and pupa ſtate, I am as yet unacquainted with.

Head obtuſe; between the eyes a beautiful, ſhining green.

Eyes, black, very large in proportion to the animal.

Antennæ, clavated, feathered, about two thirds the length of the body; below the middle, an articulation, ſuch as thoſe in the legs.

Mouth: I could not diſtinctly ſee it.

Trunk, oval, brown.

Abdomen, oblong, length of the trunk and head.

Extremities. Vide p. 232.

Legs, ſix; with them it runs briſkly, and jumps actively.

Wings, four, membranaceous, longer than the abdomen, incumbent; the anterior pair twice the ſize of the poſterior.

Tail, none.

Deſcription of the female lac inſect.

Larva, red, very minute, requiring a good lens to diſtinguiſh its parts.

Head, ſcarcely to be diſtinguiſhed from the trunk.

Antennæ, filiform, bifid, hairy, length of the inſect.

Eyes: in the back part of the trunk are two minute elevations, which may be they.

Mouth, on the middle of the breaſt, between the firſt pair of legs, which the little animal projects on being injured, otherwiſe it cannot be ſeen.

Trunk and *Abdomen*, oblong, compreſſed, tapering equally towards each end, croſſed with twelve annular ſegments, margins very flat, and ſeem to be marked with a double line.

Extremities.

Legs, ſix, running, does not jump.

Wings, none.

Tail, two ſlender white hairs, as long as the antennæ, with a white point, which may be called the rump, between them.

Pupa: the duration and peregrinations of the larvæ ſeem very ſhort and confined; for, in a few days after iſſuing from their cells they fix themſelves on the ſmall, but hard, woody branches of the tree they were produced on; it ſeeming impoſſible that they can in this ſtate tranſport themſelves to any other. About the end of December, or beginning of January, they have done iſſuing from their cells, and are ſticking faſt to the branches, regularly with their heads towards the extremity of the branch. The legs, antennæ, and tail, are now entirely gone. Their progreſs through this ſtate is ſlow, requiring about three months. Soon after they have ſettled themſelves, they become covered with a hard, brittle, garnet-coloured cruſt, ſimilar to the lac of which the cells are made, but of a brighter colour. They retain only a rude reſemblance of their former ſhape. About the end of March they have acquired three or four times their original ſize; a ſmall, round lid or cover is now obſerved at the lower part, which opens, but does not always fall off, and gives a retrograde paſſage for the fly, now in its perfect ſtate.

The inſect in its perfect ſtate is rather ſmaller than the male, of a brighter red colour, and leſs active.

Head, ſmall in proportion to the body, pointed.

Eyes, very minute.

Antennæ, filiform, not articulated as in the male, ſpreading, ſomewhat ſhorter than the inſect.

Mouth: I could not diſcover it diſtinctly.

Trunk, red, almoſt orbicular.

Abdomen, red, oblong, compoſed of twelve annular ſegments.

Extremities.

Legs, ſix, for running or jumping.

Wings, two, incumbent, longer than the abdomen, tranſparent.

Tail, two white hairs as long as the inſect.

With regard to the œconomy of theſe little animals, I muſt, for the preſent, be ſilent; having little more than conjecture to offer on that head.

The eggs, and dark-coloured glutinous liquor they are found in, communicate to water a moſt beautiful red colour, while freſh. After they have been dried, the colour they give to water is leſs bright; it would therefore be well worth while for thoſe, who are ſituated near places where the lac is plentifully found, to try to extract and preſerve the colouring principles by ſuch means as would prevent them from being injured by keeping. I doubt not but in time a method may be diſcovered to render this colouring matter as valuable as cochineal.

Mr. Hellot's proceſs for extracting the colouring matter from dry lac deſerves to be tried with the freſh lac in the month of October, or beginning of November, before the inſects have acquired life; for I found the deepeſt and beſt colour was procured from the eggs while mixed with their nidus.

His proceſs is as follows. Let ſome powdered gum lac be digeſted two hours in a decoction of comfrey root, by which a fine crimſon colour is given to the water, and the gum is rendered pale or ſtraw coloured. To this tincture, poured off clear, let a ſolution of alum be added; and when the colouring matter has ſubſided, let it be ſeparated from the clear liquor and dried; it will weigh about one fifth of the quantity of lac employed. This dried fecula is to be diſſolved or diffuſed in warm water; and ſome ſolution of tin is to be added to it, by which it acquires a vivid ſcarlet colour. This liquor is to be added to a ſolution of tartar in boiling water; and thus the dye is prepared.

In India comfrey roots are not to be had; but any other mucilaginous root, gum, or bark, would probably anſwer equally well. On ſome parts on the Coromandel coaſt, if not over it all, a decoction of the ſeeds of a very common plant (Caſſia tora of LINNÆUS), which is exceedingly mucilaginous, is uſed by the dyers of cotton cloth blue, to help to prepare the blue vat. It ſuſpends the indigo till a fermentation takes place to diſſolve it, and alſo helps to bring about that fermentation earlier than it otherwiſe would. The gum lac (or rather reſin) itſelf is known to be perfectly ſoluble in ſpirits of wine. The empty huſks which covered the pupa are alſo ſoluble in ſpirits, but without a very large proportion of the ſpirits is uſed, it ſoon becomes thick, like a jelly. Four grains communicated that quality to three drams of rectified ſpirits of wine. This jelly is very difficult of ſolution in ſpirits; a month has not effected it in a heat of from 80 to 90 degrees of FAHRENHEIT's ſcale. The ſubſtance of which theſe huſks are compoſed, is an exudation from the larvæ themſelves, which becomes hard by expoſure to the air. The cells ſeem to be made of a very different ſubſtance; what that is, and the manner in which they are made, remains ſtill to be diſcovered.

Explanation of the figures. Tab. VI.

1. A piece of lac on a ſmall branch of Mimoſa cinerea, natural ſize.

2. The outſide of the top of a cell, with its three openings; the white one with the hairs is ſtill unopened.

3. One of the utriculi for the male flies, with its two necks, which correſpond with the two upper apertures in fig. 2.

4. One of the eggs found in the utriculus, fig. 3. which produces the male flies.

5. The male fly in its perfect ſtate.

6. Small compreſſed dry grains, found in the cellulæ with the male flies. The laſt five figures are all much magnified.

7. A ſmall bit of a branch of Mimoſa cinerea, with the female inſects in their pupa ſtate, natural ſize.

8. One of the eggs which produce the female larva. They are always in that portion of the cell from whence the larva iſſues.

9. The female larva.

10. ——— pupa.

11. ——— —— with the lid opening, and the inſect protruding.

12. The female fly in its complete ſtate.

The laſt five figures are much magnified.

* * *

XVI. *The Longitudes of* Dunkirk *and* Paris *from* Greenwich, *deduced from the Triangular Meaſurement in* 1787, 1788, *ſuppoſing the Earth to be an Ellipſoid. By Mr.* Iſaac Dalby; *communicated by* Charles Blagden, *M. D. Sec. R. S.*

Read May 19, 1791.

IN the account of the Trigonometrical Operation in 1787, 1788, which is given in the Philoſophical Tranſactions, Vol. LXXX. after the diſtance of Dunkirk from the meridian of Greenwich has been determined on a parallel to the perpendicular at Greenwich, its longitude is found by ſpherical computation, on a ſuppoſition, that the ſurface of a ſphere nearly coincides with that of the earth in an eaſt and weſt direction, where the operation was performed; and the magnitude of this ſphere, or which amounts to the ſame thing, the value in parts of a degree, &c. of a meaſured arc on its ſurface (for as ſuch the arc between the meridians of Botley Hill and Goudhurſt may be conſidered) has been determined by actual obſervation at two ſtations nearly in the latitude of Dunkirk; and this independent of any hypotheſis which can ſenſibly affect the concluſion. The principles, though not ſtrictly geometrical, admit of little objection; and therefore, as much care was taken in obſerving the angles at theſe ſtations, upon which the directions of the meridians depend, the longitude of Dunkirk (and conſequently that of Paris) as given in the Table, Vol. LXXX.

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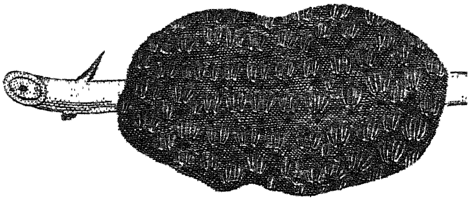

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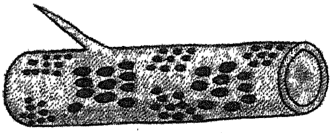

Philos. Trans. Vol. LXXXI. *Tab.* VI. *p.* 23

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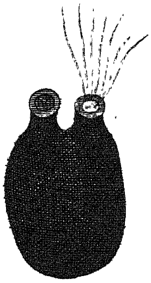

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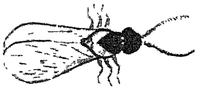

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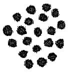

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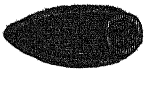

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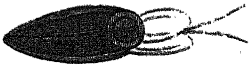

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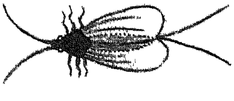

p. 232. muſt be nearly true, whatever may be the real figure of the earth. But, it may be ſaid, that the arc between the meridians of Botley Hill and Goudhurſt ($17'\frac{1}{3}$) is too ſhort to infer from obſervation the value of the arc between the meridians of Greenwich and Dunkirk (amounting to near a degree and a half), ſufficiently accurate for finding the longitude to great preciſion; becauſe it has been remarked in the Appendix to the ſame Volume, that an error of 1″, in either of the horizontal angles at the above ſtations, would cauſe a variation of near 6″ of a degree in the longitude of Dunkirk or Paris.

M. Bouguer's ſpheroid agreeing nearly with the meridional meaſurements, it was adopted for the purpoſes of latitude. But the degree perpendicular to the meridian in latitude 51° 6′ 53″ is found to be 61248 fathoms (Vol. LXXX. p. 215.) which falls ſhort of M. Bouguer's degree about 22 fathoms; therefore, ſuppoſing the directions of the meridians to have been very accurately determined, the earth cannot be this ſpheroid, notwithſtanding the ingenious hypotheſis reſpecting the curve of the meridian. But it is alſo well known, that the meaſured degrees of latitude in different places are inconſiſtent with an elliptical meridian: for, ſuppoſe an ellipſoid to be determined with the degrees found at the equator and polar circle, the computed degrees in middle latitudes will be much longer than the meaſured ones, as it is well known; and the whole meridional arc between Greenwich and Paris will, on ſuch an ellipſoid, exceed the meaſured arc by a quantity anſwering to about 21″ of latitude. It is evident, however, that if we ſuppoſe ſmall errors to have taken place in determining the celeſtial arcs, or differences of latitude in ſome of the operations (for there is little doubt but the terreſtrial menſurations in general have been made exact enough), it will be eaſy to reconcile moſt of the [illegible] to an ellipſoid.

The following computations of the longitude are made on a ſuppoſition, that the earth is an ellipſoid, for the purpoſe of comparing the concluſions with what has been inferred from obſervation. It will be ſeen, that the ratio of the axes comes out very near the ratio aſſigned by Sir ISAAC NEWTON, or 229 to 230. It is determined of ſuch a magnitude, by adhering nearly to the meaſured arc of the meridian between Greenwich and Paris, deduced from the late operation, that the computed meridional degrees differ but little from the meaſured ones in five different places in middle latitudes; but the defects at the equator and polar circle are ſuppoſed to be nearly equal to each other. This will be ſeen better by the following comparative view of the meaſured and computed degrees in the ſame latitudes.

According to	*Lat.*		*Meaſured.*	*Computed.*	*Exceſs or defect in meaſured arc.*
	°	′	Fath.	Fath.	
M CONDAMINE, &c.	0	0	60481	60344	+ 137
MASON and DIXON,	39	12	60628	60682	− 54
BOSCOVICH, &c.	43	0	60725	60738	− 13
CASSINI, &c.	45	0	60778	60768	+ 10
LIESGANIG*,	48	43	60839	60823	+ 16
French and *Engliſh*	Arc from latitude 48° 50′ 14″ to 51° 28′ 40″		160656	160662	− 6
MAUPERTUIS, &c.	66	20	61194	61057	+ 137

In the five compariſons, from latitude 39° 12′ to Greenwich, the greateſt error (54 fathoms) anſwers to about 3″ of the celeſtial arc: neither of the other four differences amounts to 1″. The determination of M. BECCARIA is not brought into the compariſon, becauſe his meaſured degree in latitude 44° 44′ is longer than the meaſured one in latitude 45°.

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* From the late Gen. ROY's Paper in the Phil. Tranſ. 1787.

The longitude of Dunkirk on this ellipſoid is found to be 9 m. 29.8 s. in time; and conſequently that of Paris 9 m. $20\frac{1}{3}$ s., which is about $1\frac{1}{2}$ s. more than that inferred from the value of the meaſured arc between Goudhurſt and the meridian of Botley Hill; and therefore the ſum of the two horizontal angles at theſe ſtations would, on this ellipſoid, be only about 4″ *leſs* than thoſe found by actual obſervation.

Method of computation.

On an ellipſoid, where the degrees of the meridian at the equator and polar circle are 60481 and 61194 fathoms reſpectively, the degree in latitude $50^\circ\ 9'\frac{1}{2}$ (the middle latitude between Greenwich and Paris) will be 60981 fathoms, exceeding the meaſured degree by 140 fathoms (Vol. LXXX. p. 225.); therefore, if each of the former degrees was about 140 fathoms leſs, the computed and meaſured arcs in latitude $50^\circ\ 9'\frac{1}{2}$ would be nearly the ſame. But, that they alſo may nearly agree in latitude 45°, let the degrees at the equator, and in latitude $50^\circ\ 9'\frac{1}{2}$, be taken 60344 and 60844; then, from theſe two degrees, the ratio of the axes will be found as the tangents of the arcs $50^\circ\ 9'\frac{1}{2}$ and $50^\circ\ 1'\ 35'\frac{1}{2}$; and the ſemi-axes 3489932 and 3473656 fathoms *.

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* Determined thus: If right lines are drawn perpendicular to the curve of a *conic ſection* to meet the axis, it is known, that *the radii of curvature at the points in the curve from whence theſe lines are drawn, will be as the cubes of theſe lines.* Hence, if PC, GB, EC (Tab. VII. fig. 1.), are perpendicular to the curve, the radii of curvature

The length of the whole meridional arc between Greenwich and Paris on this ellipſoid is ſix fathoms greater than the meaſured arc; the degree in latitude 48° 43′, 16 fathoms leſs; in latitude 45°, 10 fathoms leſs; in 43°, 13 fathoms greater; and that in latitude 39° 12′, 54 fathoms greater. The degrees at the equator and polar circle are conſiderably leſs than the meaſured ones, conformable to the hypotheſis.

Suppoſe CE, CP (fig. 1.), are the greater and leſs ſemi-axes of the ellipſoid; G Greenwich; PGE its meridian; PD the meridian of Dunkirk; and let GBA be perpendicular to the curve of the meridian at G; then GA will be the ſhorter axis of the elliptical ſection which is the perpendicular to the

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at P, G, E, will be as PC^3, GB^3, and $\overline{\frac{PC^2}{CE}}\Big|^3$, becauſe at the point E (or equator) the line ſo drawn will become the radius of curvature itſelf, or $\frac{PC^2}{CE}$. Therefore $GB^3 : \overline{\frac{PC^2}{CE}}\Big|^3$ :: *rad. curv. at* G : *rad. curv. at* E :: *length of a deg. in the lat. of* G : *length of a deg. at* E, the equator. Let the arc ERL be deſcribed with the radius CE; draw CR parallel to GB, RS parallel to PC, and join CK; then, by the nature of the ellipſe, CR (CE) : CK :: GB : *half the parameter*, or $\frac{CP^2}{CE}$; therefore $CE^3 : CK^3 :: GB^3 : \overline{\frac{CP^2}{CE}}\Big|^3$ :: 60844 : 60344 (*ſuppoſing the lat. of the point* G *to be* 50° 9′½), or CE (CR) : CK :: $\overline{60844}|^{\frac{1}{3}} : \overline{60344}|^{\frac{1}{3}}$; but CR : CK :: *ſine* SKC : *ſine* KRC (*co lat.*); therefore, $\overline{\frac{60844}{60344}}\Big|^{\frac{1}{3}} \times$ *coſine lat.* = *ſine* SKC; hence the angle SCK is given (50° 1′ 35″½); therefore, as *tang.* SCK : *tang. lat.* (SCR) :: SK : SR :: *leſſer ſemi-axis* CP : *greater* CE. And putting $d = 57{\cdot}295779$, &c. *the degrees in the circular arc which is equal to the radius*) we have $\overline{\frac{\textit{tang. lat}}{\textit{tang.}\ SCK}}\Big|^2 \times 60344\,d = 3489932$ fathoms the longer ſemi-axis; and $\frac{\textit{tang. lat.}}{\textit{tang.}\ SCK} \times 60344\,d = 3473656$, the ſhorter.

meridian at Greenwich, and the angle EBG will be the latitude of Greenwich, or 51° 28′ 40″. Let HO (parallel to GA) be the ſection of the parallel to that perpendicular, paſſing through Dunkirk. Then by the Table, p. 232. (Vol. LXXX.) the arc GH is 152549 feet; but this arc exceeds the real diſtance of the parallels GA, HO, not more than a fathom; therefore this diſtance may be taken = 25424 fathoms. Now the ſections GA, HO, of the ellipſoid being ſimilar, from the known properties of the figure, we ſhall get HO the ſhorter axis of the ſection of the parallel = 6959396, its longer axis = 6979374, and HW = 3531757 fathoms, W being the point where HO cuts the axis PI of the ellipſoid. Hence, if D be Dunkirk, and the arc HD the meaſured arc of the parallel, we have given the length of this arc, or 547058 feet (Table, p. 232.) = 91176 fathoms, and alſo the point W in the leſſer axis of the ſection HO, to determine the angle HWD in the plane of this ſection. But reverting the ſeries which exhibits the length of an elliptical arc in terms of the *abſciſs* and *ordinate*, will be of little uſe in the preſent caſe, where the arc and its chord are very near of the ſame length: For, let HKOL (fig. 2.) be the ſection of the parallel, where HO = 6959396, and KL = 6979374, are the axes; and HW = 3531757, as in *fig.* 1.; alſo, ſuppoſe HS is the radius of curvature at H, or at the middle of HD; then, if we conceive the arc HD to be a right line, or deſcribed with the radius HW, or with HS (3499700) and thence determine the angle SWD from the two ſides SD, SW, and the included angle (the ſupplement of HSD); in either caſe we get the angle HWD the ſame, or 1° 28′ 44″,8 to within 1″. This angle being obtained, the inclination of the planes PHW, PDW (the planes of the meridians of

Greenwich and Dunkirk, *fig.* 1.), or the longitude of the point D, will be found by the common proportion which in a right-angled ſpherical triangle determines an angle when the legs are given: this will be obvious by conceiving a ſphere (of any magnitude) to be deſcribed about W as a center.

Hence, as *rad.* : *cotang.* angle HWD (1° 28′ 44″.8) :: *ſine* angle HWP (38° 31′ 20″) : *cotang.* 2° 22′ 26″½, the inclination of the planes of the meridians PH, PD, or longitude of Dunkirk on this ellipſoid. And as the difference of meridians of Paris and Dunkirk is 2′ 21″.9 (for this will not be materially affected by different hypotheſes) the longitude of Paris will be 9 m. 20⅓ s. in time. The longitude of Dunkirk from Paris (2′ 21″.9) is the mean longitude deduced at p. 223. (Vol. LXXX.), which is only 1″.1 leſs than that given in the *Connoiſſance des Temps*, 1788.

The method of computing the latitude of the point D (was it neceſſary) is thus: as *rad.* : *coſine* DWH :: *coſine* HWP : *coſine* DWP; and ſince the point W in the axis PW is given, and alſo the angle DWP in the plane of the meridian PD (by the foregoing proportion), the point D will be determined by the properties of the ellipſe; which in fact is nothing more than finding the inclination of the vertical at the point D with the given line DW, which inclination added to the angle DWP, gives the co-latitude of the point D. And hence may be evinced the truth of what is advanced at p. 199. (Vol. LXXX.), that *if the value of an arc on a ſpheroid, conſidered as an arc of a great circle perpendicular to the meridian, be given, the longitude may be found by ſpherical computation, but not the latitude.* For conceive the arc HD to be perpendicular to the meridian at H, then the angle HWP would be the co-latitude

of

of the point H; and the former proportion would give the longitude of D, whether the figure was a ſphere or ſpheroid; and the angle DWP (found by the latter proportion) wou'd be the co-latitude of D ſuppoſing it a ſphere, in which caſe the point W becomes the center; but this will not hold in a ſpheroid, becauſe DW would not be perpendicular to the meridian at D.

The forgoing method of computing the longitude from the meaſured arc of a parallel on a given ellipſoid (though evidently the direct one), will be tedious, eſpecially when the lengths of the meaſured arcs (GH, HD) are very conſiderable. But when the latitude of the point H is determined from the meaſured arc GH (on the known meridian), and the extent of the other arc (HD), or rather the angle HWD, is not more than two or three degrees, the ſame concluſions, extremely near, may be obtained in the following manner, which is nearly the ſame as the method uſed in computing the longitudes in the Table of General reſults, p. 232. (Vol. LXXX.).

Suppoſe G and D (fig. 1.) to be Greenwich and Dunkirk; PH, PD, their meridians, as before; and let HD (inſtead of its being a parallel to the perpendicular at Greenwich) be an arc of an ellipſe cutting the meridian of Greenwich at right angles, ſuppoſe in the point H. Then the arc GH being = 152549 + 50 feet nearly (becauſe the ellipſe which paſſes through D, and is at right angles to the meridian PG, will fall about 50 feet to the ſouth of the point cut by the parallel), therefore the value of the arc GH, or 25433 fathoms, will, on this ellipſoid, be 25′ 4″.4, and conſequently the angle PWH, or the co-latitude of H, is 38° 56′ 24″.4. Now, the radius of curvature of this perpendicular ellipſe at H, the extremity of its leſſer axis, will be 3499798 fathoms[*], which, divided by 57.295779, &c. (the degrees in the circular arc which is equal to the radius), gives 61083 fathoms for a degree on this ellipſe, conſidered as a great circle perpendicular to the meridian at the point H on the ellipſoid; and ſince the length of this arc (HD) will be nearly the ſame as that of the parallel, or 91176 fathoms, its value will be 1° 29′ 33″.6 (the arc DH, or rather the angle DWH). Hence, as *rad.* : *cotang.* 1° 29′ 33″.6 (HWD) :: *ſine* 38° 56′ 24″.4 (HWP) : *cotang.* 2° 22′ 26″.8, the longitude of D, or Dunkirk, the ſame as before, very near; hence the longitude of Paris will be 2° 20′ 4″.9. But the ſame may be obtained from the mean diſtance of the meridians of Greenwich and Paris, or 537950 feet. See p. 599. in the Appendix to Vol. LXXX.

It appears from the foregoing hypotheſis, that the meaſured degrees of the meridian in middle latitudes will anſwer nearly on an ellipſoid whoſe axes are in the ratio aſſigned by Sir ISAAC NEWTON. But this will receive further confirmation from the fifth ellipſoid in the ſecond Table, p. 232. (Vol. LXXX.), where the near agreement between the computed and meaſured arc of the meridian between Greenwich and Perpignan (differing but about 52 fathoms in the extent of 8° 46′ 44″) would be ſomewhat extraordinary, were we certain that the latitude of Perpignan (42° 41′ 56″) is correct; but this is ſuſpected by M. DE LA

CAILLE

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[*] It is not neceſſary to determine the axes of this ellipſe, becauſe when HW is perpendicular to the curve of the meridian, it will (by the nature of the figure) be the radius of curvature of the arc HD at the point H. Hence, if we put v for the *cotang.* and c for the *coſine* of the latitude of the point H, and let a denote the *ſine* of an arc whoſe *tang.* is $\frac{CE}{CP} \times v$; then $\frac{a}{c} \times CE = HW$, by the properties of the ellipſe.

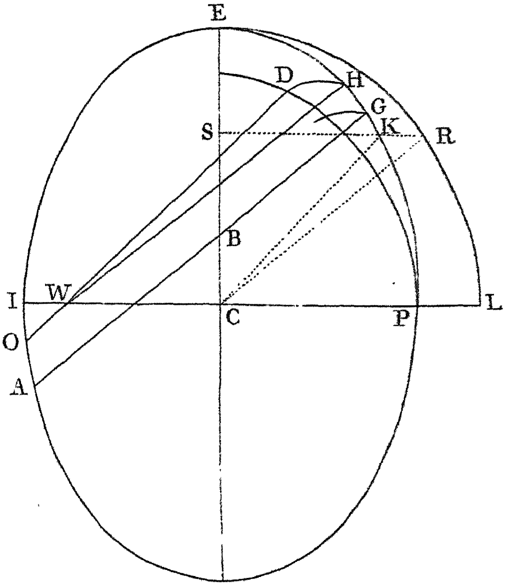

Fig. 1.

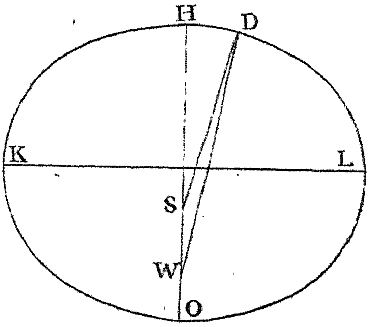

Fig. 2.

CAILLE. See *Mem. de l'Acad.* 1758. The computed arc, however, between Greenwich and Paris is 19 fathoms longer than the meaſured arc, which anſwers to a little more than 1″ of latitude.

The longitude of Paris on this ellipſoid is 9 m. 20$\frac{4}{10}$ s.

It it be contended, that the operations at the equator and polar circle were as correct as thoſe executed for the like purpoſe in middle latitudes; and that a kind of mean between the extreme reſults ought to be preferred; we ſhall ſtill get an ellipſoid, whoſe axes are nearly as 229 to 230, by taking the degrees at the equator and polar circle each 70 fathoms leſs, and that in latitude 50° 9 $\frac{1}{2}$ as much greater than the meaſured ones; and the longitude of Paris will be found 9 m. 19$\frac{7}{10}$ s. But the computed meridional arc between Greenwich and Paris will exceed the meaſured one by a quantity anſwering to about 11″ of latitude.

It is almoſt needleſs to obſerve, that the longitude of Paris (9 m. 20 s.) deduced by Dr. MASKELYNE from the different reſults found by aſtronomical obſervations (Phil. Tranſ. 1787) agrees to leſs than half a ſecond with either of the above determinations.

N. B. In the Table of General Reſults, p. 232. Vol LXXX. *read* 18′ 46″ for the longitude of Wrotham Hill; this ſhould have been corrected in the Appendix.

⁂

XVII. *On the Method of determining, from the real Probabilities of Life, the Values of contingent Reverſions in which Three Lives are involved in the Survivorſhip. By Mr.* William Morgan, *F. R. S.*

Read May 26, 1791.

HAVING been encouraged to the further purſuit of the doctrine of ſurvivorſhips by the very honourable manner in which my two former Papers on this ſubject were received by the Royal Society, I think it my duty to ſubmit the reſult of my labours to their conſideration. The ſolutions of ſome of the following problems might have been derived from thoſe which I have already communicated; but the direct inveſtigation of each ſeparate problem being certainly more ſatisfactory, and the rules obtained by this means in general more ſimple, I have conſidered no problem as connected with another, except the relation between them either immediately ariſes from the ſolution, or is neceſſary to prove the truth of it. Being anxious to render myſelf as conciſe as poſſible, I have been minute only in the inveſtigation of the firſt problem, and have done little more than ſtate the contingencies which will determine the ſurvivorſhip in the others. By the aſſiſtance, however, of theſe, and the operations which are detailed in my former Papers, the theorems which I have given may be deduced without much difficulty.

In order to prevent unneceſſary repetitions, it may not be improper to begin with explaining the different ſymbols which are uſed in the following pages.

A, B, C, denote the value of an annuity on the reſpective lives of A, B, or C.

D, denotes the value of S on the contingency of C's ſurviving A (by my 2d prob. Vol. LXXVIII).

E, denotes the ſame value on the contingency of B's ſurviving A, found by the ſame problem.

F, denotes the value of an annuity on a life one year younger than B.

G, denotes the value of the abſolute reverſion of S after the death of A.

H, denotes the value of an annuity on a life one year younger than A.

K, denotes the value of an annuity on a life one year younger than C.

L, denotes the value of an annuity on the longeſt of the three lives of A, B, and C.

M, denotes the value of S by the firſt problem on the contingency that A's life ſhall be the *firſt* that fails.

N, denotes the value of an annuity on a life one year older than A.

P, denotes the value of an annuity on a life one year older than B.

R, denotes the value of S on the contingency of B's dying after A (by my 3d prob. Vol. LXXVIII).

S, denotes the given ſum.

T, denotes the value of an annuity on a life one year older than C.

V, denotes the perpetuity.

W, denotes the value of S on the contingency of C's dying after A (by my 3d prob. Vol. LXXVIII).

α and a denote the number of perſons living in a table of obſervations at the ages of H and A.

s, t, u, w, &c. denote the number of perſons living at the end of the 1ſt, 2d, 3d, &c. years from the age of A.

β and b, denote the number of perſons living at the ages of F and B.

m, n, o, p, &c. denote the number of perſons living at the end of the 1ſt, 2d, 3d, &c. years from the age of B.

$\varkappa$ and c denote the number of perſons living at the ages of K and C.

d, e, f, g, &c. denote the number of perſons living at the end of the 1ſt, 2d, 3d, &c. years from the age of C.

$$\left.\begin{array}{l} a', a'', a''', \&c. \\ b', b'', b''', \&c. \\ c', c'', c''', \&c. \end{array}\right\}$$ denote the decrements of life at the end of the 1ſt, 2d, 3d, &c. years from the reſpective ages of A, B, and C.

r, denotes the value of £. 1 increaſed by its intereſt for a year.

The combinations of two or three of the ſeveral letters A, B, C, F, H, &c. denote the values of annuities on the joint continuance of two or three of thoſe reſpective lives.

PROBLEM I.

To determine the value of a given ſum, payable if A ſhould be the *firſt* that fails of the three lives A, B, and C.

SOLUTION.

In order to receive the given ſum in the firſt year, it is neceſſary that one or other of four events ſhould happen. 1ſt, That all the three lives ſhould fail, and that A ſhould die firſt. 2dly, That B ſhould die *after* A, and C live. 3dly, That C ſhould die *after* A, and B live. 4thly, That A only ſhould die, and B and C both live. Theſe ſeveral contingencies being expreſſed by $\frac{a'.\overline{b-m}.\overline{c-d}}{3abc}$, $\frac{a'.\overline{b-m}.d}{2abc}$, $\frac{a'.\overline{c-d}.m}{2abc}$, and $\frac{a'.dm}{abc}$, reſpectively, their ſum will be $=\frac{1}{abc}\times\overline{\frac{a'bc}{3}+\frac{a'md}{3}+\frac{a'mc}{6}+\frac{a'bd}{6}}$. In the ſecond and following years one or other of the ſame events muſt take place in order to receive the given ſum; that is, they muſt either all three die, A dying firſt; or only A and B muſt die, A dying firſt; or only A and C muſt die, A dying firſt; or only A muſt die, and B and C both live. The different fractions expreſſing thoſe four contingencies for the ſecond year being $\frac{a''.\overline{m-n}.\overline{d-e}}{3abc}$, $\frac{a''.\overline{m-n}.e}{2abc}$, $\frac{a''.\overline{d-e}.n}{2abc}$, and $\frac{a''.ne}{abc}$; for the third year $\frac{a'''.\overline{n-o}.\overline{e-f}}{3abc}$, $\frac{a'''.\overline{n-o}.f}{2abc}$, $\frac{a'''.\overline{e-f}.o}{2abc}$, and $\frac{a'''.of}{abc}$, and ſo on, the whole value of the given ſum will be $=\frac{S.a'}{abcr}\times\overline{\frac{bc}{3}+\frac{md}{3}+\frac{mc}{6}+\frac{bd}{6}}+\frac{S.a''}{abcr^2}\times\overline{\frac{md}{3}+\frac{ne}{3}+\frac{nd}{6}+\frac{me}{6}}+\frac{S.a'''}{abcr^3}\times\overline{\frac{ne}{3}+\frac{of}{3}+\frac{oe}{6}+\frac{nf}{6}}+\&c.$ which are $=\frac{S}{3abc}\times\overline{\frac{a'bc}{r}+\frac{a''md}{r^2}+\frac{a'''.en}{r^3}+\&c.}+\frac{S}{6abc}\times\overline{\frac{a'mc}{r}+\frac{a''nd}{r^2}+\frac{a'''.oe}{r^3}+\&c.}+\frac{S}{6abc}\times\overline{\frac{a'db}{r}+\frac{a''em}{r^2}+\frac{a'''.fn}{r^3}+\&c.}+\frac{S}{3abc}\times\overline{\frac{a'md}{r}+\frac{a''.en}{r^2}+\frac{a'''fo}{r^3}}+\&c.$ From the demonſtration of the problem in my laſt Paper * it appears, that the firſt of theſe ſeries is $=\frac{S}{3}\times\frac{\beta x.\overline{FK-AFK}}{bc}-\frac{S}{3r}\times\overline{BC-ABC}$; that the ſecond ſeries is $=\frac{S}{6}\times\frac{x.\overline{BK-ABK}}{c}-\frac{S}{6r}\times\frac{m.\overline{PC-APC}}{b}$; that the

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* See Phil, Tranſ. Vol LXXIX.

third ſeries is $= \frac{S}{6} \times \frac{\beta\ \overline{FC - AFC}}{b} - \frac{S}{6r} \times \frac{d\,.\,\overline{B\Gamma - AB\Gamma}}{c}$; and that the fourth ſeries is $= \frac{S}{3} \times \overline{BC - ABC} - \frac{S}{3} \times \frac{md\,.\,\overline{PT - AP\Gamma}}{bc}$. Theſe ſeveral expreſſions being added together will be found $=$ S into $\frac{x}{3c} \times \overline{\frac{\beta\,.\,\overline{FK - AFK}}{b} + \frac{BK - ABK}{2}} + \frac{\beta}{6b} \times \overline{FC - AFC} + \frac{\overline{r-1}}{3r} \times \overline{BC - ABC}$ $- \frac{m\,.\,\overline{PC - APC}}{6br} - \frac{d}{3cr} \times \overline{\frac{B\Gamma - AB\Gamma}{2} + \frac{m\,.\,\overline{PT - AP\Gamma}}{b}}$.

This theorem gives the exact value when either B or C are the oldeſt of the three lives; but when A is the oldeſt, it will be neceſſary to exchange the ſymbols a', a'', a''', &c. for $\overline{a-s}$, $\overline{s-t}$, $\overline{t-u}$, &c. and the ſymbols m, n, o, &c. for $\overline{c-c'}$, $\overline{c-c'+c''}$, $\overline{c-c'+c''+c'''}$, &c. In this caſe the value of the given ſum for the firſt year will be found $= \frac{S}{abcr}$ into $\frac{abc}{2} - \frac{bsc}{2} + \frac{amc}{2} - \frac{msc}{2} - \frac{abc'}{6} + \frac{bsc'}{6} - \frac{amc'}{3} + \frac{msc'}{3}$; for the ſecond year $= \frac{S}{abcr^2}$ into $\frac{msc}{2} - \frac{mtc}{2} + \frac{nsc}{2} - \frac{ntc}{2} - \frac{msc''}{6} + \frac{mtc''}{6} - \frac{nsc''}{3} + \frac{ntc''}{3} - \frac{m\ c'}{2} + \frac{mtc'}{2} - \frac{nsc'}{2} + \frac{ntc'}{2}$; for the third year $= \frac{S}{abcr^3}$ into $\frac{ntc}{2} - \frac{nuc}{2} + \frac{otc}{2} + \frac{cuc}{2} - \frac{ntc'''}{6} + \frac{nuc'''}{6} - \frac{otc'''}{3} + \frac{ouc'''}{3} - \frac{nt\,.\,\overline{c'+c''}}{2} + \frac{nu\,.\,\overline{c'+c''}}{2} - \frac{ot\,.\,\overline{c'+c''}}{2} + \frac{cu\,.\,\overline{c'+c''}}{2}$; and ſo on for the remaining years of A's life. Hence the whole value of the given ſum will be $= \frac{S}{2ab} \times \overline{\frac{ab}{r} + \frac{ms}{r^2} + \frac{nt}{r^3} + \&c.}$ $- \frac{S}{2ab} \times \overline{\frac{bs}{r} + \frac{mt}{r^2} + \frac{nu}{r^3} + \&c.} + \frac{S}{2ab} \times \overline{\frac{am}{r} + \frac{ns}{r^2} + \frac{ot}{r^3} + \&c.} - \frac{S}{2ab} \times$ $\overline{\frac{ms}{r} + \frac{nt}{r^2} + \frac{ou}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{abc'}{r} + \frac{msc''}{r^2} + \frac{ntc'''}{r^3} + \&c.} - \frac{S}{2abcr} \times$ $\overline{\frac{msc'}{r} + \frac{nt\,.\,\overline{c'+c''}}{r^2} + \&c.} + \frac{S}{6abc} \times \overline{\frac{bsc'}{r} + \frac{mtc''}{r^2} + \frac{nuc'''}{r^3} + \&c.} + \frac{S}{2abcr} \times$

mtc'

$\overline{\frac{mtc'}{r}+\frac{nu\,.\,\overline{c'+c''}}{r^2}+\&c.}-\frac{S}{3abc}\times\overline{\frac{amc'}{r}+\frac{ns\,.\,c''}{r^2}+\frac{ot\,.\,c'''}{r^3}+\&c.}-\frac{S}{2abcr}\times$ $\overline{\frac{nsc'}{r}+\frac{ot\,.\,\overline{c'+c''}}{r^2}+\&c.}+\frac{S}{3abc}\times\overline{\frac{msc'}{r}+\frac{nt\,.\,c''}{r^2}+\frac{ouc'''}{r^3}+\&c.}\times\frac{S}{2abcr}\times$ $\overline{\frac{nt\,.\,c'}{r}+\frac{ou\,.\,\overline{c'+c''}}{r^2}+\&c.}$ . . . The firſt four ſeries are reſpectively $=$ $\frac{\alpha\beta\,.\,HF}{2ab}$ (or $\frac{1+AB}{2r}$) $-\frac{\beta\,.\,AF}{2b}+\frac{\alpha\,.\,HB}{2a}-\frac{AB}{2}$ . . . The firſt term of the fifth ſeries, or $\frac{abc'}{r}$, is $=\frac{\alpha\beta}{abr}\times\overline{\frac{ab}{\alpha\beta}-\frac{ab\,.\,\overline{c-c'}}{\alpha\beta c}}$, the ſecond term or $\frac{msc''}{r^2}$, is $=\frac{\alpha\beta}{abr^2}\times\overline{\frac{ms}{\alpha\beta}-\frac{ms\,.\,\overline{c-\overline{c'+c''}}}{\alpha\beta c}}-\frac{msc'}{r^2}$ . . . . the third term, or $\frac{ntc'''}{r^3}$, is $=\frac{\alpha\beta}{abr^3}\times\overline{\frac{nt}{\alpha\beta}-\frac{nt\,.\,\overline{c-\overline{c'+c''+c'''}}}{\alpha\beta c}}-\frac{nt'\,.\,\overline{c'+c''}}{r^3}$. Therefore the ſum of the fifth and ſixth ſeries is $=-\frac{\alpha\beta\,.\,\overline{HF-HFC}}{6ab}\left(-\frac{msc'}{3abcr^2}-\frac{nt\,.\,\overline{c'+c''}}{3abcr^3}-\&c.=\right)-\frac{1}{3r}\times\overline{AB-ABC}$. In the ſame manner the ſeventh ſeries may be found $=\frac{\beta}{6b}\times\overline{AF-AFC}-\frac{mtc'}{6abcr^2}-\frac{nu\,.\,\overline{c+c''}}{6abcr^3}-\&c.$; and conſequently the ſum of the ſeventh and eighth ſeries is $=\frac{\beta\,.\,\overline{AF-AFC}}{6b}+\frac{s\,.\,\overline{BN-BNC}}{3ar}$. Again, the ninth and tenth ſeries may be found $=-\frac{\alpha\,.\,\overline{HB-HBC}}{3a}\left(-\frac{nsc'}{6abcr^2}-\frac{ot\,.\,\overline{c'+c''}}{6abcr^3}-\&c.=\right)-\frac{m\,.\,\overline{AP-APC}}{6br}$; and the eleventh and twelfth or two laſt ſeries, $=\frac{\overline{AB-ABC}}{3}\left(+\frac{ntc'}{6abcr^2}+\frac{uo\,.\,\overline{c'+c''}}{6abcr^3}+\&c.=\right)+\frac{ms\,.\,\overline{PN-PNC}}{6abr}$. If all theſe expreſſions be added together, we ſhall have the value of the given ſum $=$ S into $\frac{\beta}{3b}\times\overline{\frac{\alpha\,.\,\overline{HF+\frac{1}{2}HFC}}{a}-\overline{AF+\frac{1}{2}AFC}}$

$$+\frac{1}{6}\times\overline{\frac{\alpha . \overline{HB}+2\overline{HBC}}{a}-\overline{AB}+2ABC}+\frac{1}{3}-\overline{\frac{s . \overline{BN-BNC}}{a}-AB-\overline{ABC}}$$

$$+\frac{m}{6ar}\times\overline{\frac{s . \overline{PN-PNC}}{a}-\overline{AP-APC}}.$$

It may be obſerved, that the firſt four ſeries are alſo $=E$, and therefore if this value be ſubſtituted inſtead of $\frac{\alpha 3 . HF}{2ab}$, &c. the general rule will become $=E+S$ into $\overline{-\frac{\alpha}{3a}\times\overline{HB-HBC}+\frac{\beta}{2b}\times\overline{HF-HFC}+\frac{\overline{r-1} . \overline{AB-ABC}}{3r}+\frac{\beta . \overline{AF-AFC}}{6b}-\frac{m . \overline{AP-APC}}{6br}+\frac{s}{3ar}\times\overline{BN-BNC}+\frac{2r}{m}\times\overline{PN-PNC}}$ ...... Suppoſing the three lives to be equal, the firſt of theſe rules will become $=\frac{1+CC}{2r}-\frac{\varkappa\varkappa . \overline{KK-CKK}}{6cc}-\frac{1}{3r}\times\overline{CC-CCC}-\frac{\varkappa . \overline{CK-CCK}}{6c}+\frac{d . \overline{CT-CCT}}{6cr}-\frac{\overline{CC+2CCC}}{6}+\frac{dd . \overline{TT-CTT}}{6 . cc . r}$, and the ſecond $=\frac{\overline{r-1} . \overline{V-CC}}{2r}+\frac{\overline{r-1} . \overline{CC-CCC}}{3r}-\frac{\varkappa . \overline{CK-CCK}}{6c}-\frac{\varkappa\varkappa . \overline{KK-CCK}}{6cc}+\frac{d . \overline{CT-CCT}}{6cr}+\frac{dd . \overline{TT-CTT}}{6ccr}$: and alſo the rule denoting the value (when either B or C are the eldeſt of the three lives) will become $=\frac{\overline{r-1} . \overline{CC-CCC}}{3r}+\frac{\varkappa}{3c}\times\overline{CK-CCK}+\frac{\varkappa\varkappa}{3 . cc}\times\overline{KK-CKK}-\frac{d}{c}\times\frac{\overline{CT-CCT}}{3r}-\frac{dd .}{3ccr}\times\overline{TT-CTT}$. Let this laſt expreſſion be called Q and compared with the firſt of the preceding expreſſions. In this caſe we ſhall have $\frac{1+CC}{2r}-\frac{\overline{CC+2CCC}}{6}-\frac{\overline{CC-CCC}}{3r}-\frac{Q}{2}+\frac{\overline{r-1} . \overline{CC-CCC}}{6r}=Q$, from which Q may be eaſily found $=\frac{S}{3}\times\frac{\overline{r-1} . \overline{V-CCC}}{r}$. Again, let the ſame expreſſion, denoted by Q, be compared with the ſecond of the preceding expreſſions, and we ſhall then have $\frac{\overline{r-1}\,.\,\overline{V-CC}}{2r} - \frac{Q}{2} + \frac{\overline{r-1}\,.\,\overline{CC-CCC}}{2r} = Q$, and conſequently $Q = S \times \frac{\overline{r-1}\,.\,\overline{V-CCC}}{3r}$ as before* .... Now it is well known, that when the lives are all equal, the value of the reverſion muſt be *one third* the difference between the perpetuity and the three joint lives, and therefore a demonſtration ariſes of the truth of the whole ſolution. As a ſtill further proof of this, the foregoing theorem may be immediately deduced from the ſeries themſelves: thus, the value of the given ſum for the firſt year will in this caſe be $= \frac{S\,.\,\overline{c^3-a^3}}{3c^3\,.\,r}$; for the ſecond year it will be $= \frac{S\,.\,\overline{d^3-e^3}}{3c^3r^2}$; for the third year it will be $= \frac{S\,.\,\overline{e^3-f^3}}{3c^3r^3}$. Hence the value of the whole reverſion will be $= \frac{S}{3} \times \overline{\frac{1}{r} - \frac{d^3}{c^3r} - \frac{e^3}{c^3r^2} - \text{\&c.}} + \frac{S}{3} \times \overline{\frac{d^3}{c^3r^2} + \frac{e^3}{c^3r^3} + \frac{f^3}{c^3r^4} + \text{\&c.}} = \frac{S}{3} \times \overline{\frac{1}{r} - CCC + \frac{CCC}{r}} = \frac{S}{3} \times \frac{\overline{r-1}\,.\,\overline{V-CCC}}{r}$ ... Q. E. D.

PROBLEM II.

To determine the value of a given ſum, payable if A ſhould be the *ſecond* that fails of the three lives A, B, and C.

SOLUTION.

The ſum S may be received in the firſt year, provided either of three events ſhould happen; 1ſt, if the three lives ſhould become extinct, and A be the ſecond that fails; 2dly, if A ſhould die after B, and C live; and, 3dly, if A ſhould die after C, and B live. But in the ſecond and following years, the given ſum may be received, provided either of ſeven events ſhould happen. 1ſt, If the three lives ſhould fail in the year, A's life having been the ſecond that failed. 2dly, If A only ſhould die in the year, B having died before the beginning and C lived to the end of it. 3dly, If A only ſhould die in the year, C having died before the beginning and B lived to the end of it. 4thly, If A ſhould die after B in the year and C live. 5thly, If A ſhould die after C in the year and B live. 6thly, if A and C ſhould both die in the year (A dying firſt) and B's life ſhould have failed in one or other of the foregoing years; and, 7thly, if A and B ſhould both die in the year (A dying firſt) and C's life ſhould have failed in the foregoing years. From the ſeveral expreſſions denoting theſe contingencies the whole value of the reverſion may be found $=$

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* This expreſſion may alſo be obtained from either of the above general rules, independent of the two others, in like manner as in the ſolution of the fourth problem.

$$\frac{S}{2ac} \times \overline{\frac{a'c}{r} + \frac{a''d}{r^2} + \frac{a'''e}{r^3} + \&c.} + \frac{S}{2ac} + \overline{\frac{a'd}{r} + \frac{a''e}{r^2} + \frac{a'''f}{r^3} + \&c.} +$$

$$\frac{S}{2ab} \times \overline{\frac{a'b}{r} + \frac{a''m}{r^2} + \frac{a'''n}{r^3} + \&c.} + \frac{S}{2ab} \times \overline{\frac{a'm}{r} + \frac{a''n}{r^2} + \frac{a'''o}{r^3} + \&c.} -$$

$$\frac{2S}{3abc} \times \overline{\frac{a'bc}{r} + \frac{a''md}{r^2} + \frac{a''ne}{r^3} + \&c.} - \frac{2.S}{3abc} \times \overline{\frac{a'md}{r} + \frac{a''ne}{r^2} + \frac{a'''of}{r^3} + \&c.} -$$

$$\frac{S}{3abc} \times \overline{\frac{a'mc}{r} + \frac{a''na}{r^2} + \frac{a'''oe}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{a'bd}{r} + \frac{a''me}{r^2} + \frac{a''nf}{r^3} + \&c.}$$

The two firſt of theſe ſeries are $=$ D, . . the two next $=$ E . . . and it appears from the ſolution of the preceding problem that the four remaining ſeries expreſs double the value of the ſum S, depending on the contingency of A's dying firſt (when B or C are the oldeſt of the three lives) with a negative ſign.

The general rule, therefore, in this caſe will become $= D + E - 2M$.

But when A is the oldeſt life, recourſe muſt be had, as in the ſecond part of the preceding problem, to different ſymbols, and the value of the reverſion will then be found $=$

$$\frac{S}{ab}\times-\overline{\frac{ab}{r}+\frac{ms}{r}+\frac{nt}{r^3}}+\&c.+\frac{S}{ab}\times\overline{\frac{bs}{r}+\frac{mt}{r^2}+\frac{nu}{r^3}+\&c.}+$$
$$\frac{S}{3abc}\times\overline{\frac{abc'}{r}+\frac{msc''}{r^2}+\frac{ntc'''}{r^3}}+\&c.-\frac{S}{3abc}\times\overline{\frac{bsc'}{r}+\frac{mtc''}{r^2}+\frac{nuc'''}{r^3}+\&c.}+$$
$$\frac{2S}{3abc}\times\overline{\frac{amc'}{r}+\frac{nsc''}{r^2}+\frac{otc'''}{r^3}}+\&c.-\frac{2S}{3abc}\times\overline{\frac{msc'}{r}+\frac{ntc''}{r^2}+\frac{ouc'''}{r^3}+\&c.}+$$
$$\frac{S}{abc}\times\overline{\frac{msc'}{r}+\frac{nt\cdot\overline{c'+c''}}{r^2}}+\&c.-\frac{S}{abc}\times\overline{\frac{mtc'}{r}+\frac{nu\cdot\overline{c'+c''}}{r^2}+\&c.}+$$
$$\frac{S}{abc}\times\overline{\frac{nsc'}{r}+\frac{ot\cdot\overline{c'+c''}}{r^2}}+\&c.-\frac{S}{abc}\times\overline{\frac{nt\cdot c'}{r}+\frac{ou\cdot\overline{c'+c''}}{r^2}+\&c.}+$$
$$\frac{S}{2ab}\times\overline{\frac{\overline{a-s}\cdot b}{r}+\frac{\overline{s-t}\cdot m}{r^2}+\frac{\overline{t-u}\cdot n}{r^3}}+\&c.+\frac{S}{2ab}\times\overline{\frac{\overline{a-s}\cdot m}{r}+\frac{\overline{s-t}\cdot n}{r^2}+\&c.}+$$
$$\frac{S}{2ac}\times\overline{\frac{\overline{a-s}\cdot c}{r}+\frac{\overline{s-t}\cdot\overline{c-c'}}{r^2}}+\&c.+\frac{S}{2ac}\times\overline{\frac{\overline{a-s}\cdot\overline{c-c'}}{r}+\frac{\overline{s-t}\cdot\overline{c-c'+c''}}{r^2}+\&c.}-\frac{S}{ab}\times\overline{\frac{am}{r}+\frac{ns}{r^2}+\frac{ot}{r^3}+\&c.}+\frac{S}{ab}\times\overline{\frac{ms}{r}+\frac{nt}{r^2}+\frac{ou}{r^3}}+\&c.$$

The laſt two, and the firſt ten ſeries are $= -2M$, the eleventh and twelfth ſeries are $= D$, and the thirteenth and fourteenth ſeries $= E$; conſequently the general rule becomes $= D + E - 2M$, as before.

Suppoſing the lives were all equal, the above expreſſion would be $= \frac{S\cdot\overline{r-1}\cdot\overline{V-CC}}{r}-\frac{2\cdot S}{3r}\times\overline{r-1}\cdot\overline{V-CCC}=\frac{S}{3}\times\frac{\overline{r-1}}{r}\times\overline{V-3CC-2CCC}$, which is known from other principles* to be the true value, and therefore the inveſtigation is right. As a further demonſtration, however, it may not be improper to obſerve, that this rule is immediately obtained from the different fractions which expreſs the ſeveral contingencies in each year. For in this caſe, the value of S for the firſt year becomes $=\frac{S}{r}\times\overline{\frac{1}{3}-\frac{dd}{cc}+\frac{2d^3}{c^3}}$, for the ſecond year $=\frac{S}{r^2}\times\overline{\frac{dd}{cc}-\frac{ee}{cc}-\frac{2d^3}{c^3}+\frac{2e^3}{c^3}}$ and ſo on for the other years. Theſe ſeries being added together will be found $=\frac{S}{3}\times\overline{\frac{1}{r}+\frac{2d^3}{c^3\,.\,r}+\frac{2e^3}{c^3\,.\,r^2}+\&c.}$ $-\frac{S}{3}\times\overline{\frac{3dd}{ccr}+\frac{3ee}{cc\,.\,r^2}+\&c.}+\frac{S}{3r}\times\overline{\frac{3dd}{ccr}+\frac{3ee}{cc\,.\,r^2}+\&c.}-\frac{S}{3r}\times\overline{\frac{2d^3}{c^3\,.\,r^2}+\frac{2e^3}{c^3\,.\,r^2}+\&c.}=\frac{S}{3}\times\frac{r-1}{r}\times\overline{V-3CC-2CCC}$ .... Q. E. D.

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* See Phil. Tranſ. Vol. LXXIX.

PROBLEM III.

To determine the value of a given ſum payable on the death of A, if his life ſhould be the *laſt* that fails of the three lives A, B, C.

SOLUTION.

The given ſum can be received in the firſt year only upon the extinction of the three lives, A having died laſt. In the ſecond and following years it may be received provided either of four events ſhould happen: 1ſt, if all the three lives ſhould fail in the year, A dying laſt; 2dly, if A ſhould die *after* C in the year, B having died in either of the foregoing years; 3dly, if A ſhould die *after* B in the year, C having died in either of the foregoing years; 4thly, if only A ſhould die in the year, B and C having both died before the beginning of it. The value therefore of the reverſion (when B or C are older

than A) will be $= \frac{S}{a} \times \overline{\frac{a'}{r} + \frac{a''}{r^2} + \frac{a'''}{r^3} + \&c.} + \frac{S}{3abc} \times \overline{\frac{a'bc}{r} + \frac{a''md}{r^2} + \frac{a'''ne}{r^3}}$
$\overline{+ \&c.} + \frac{S}{3abc} \times \overline{\frac{a'md}{r} + \frac{a''ne}{r^2} + \frac{a'''of}{r^3} + \&c.} + \frac{S}{6abc} \times \overline{\frac{a'mc}{r} + \frac{a''nd}{r^2} + \frac{a'''oe}{r^3}}$
$\overline{+ \&c.} + \frac{S}{6abc} \times \overline{\frac{a'bd}{r} + \frac{a''me}{r^2} + \frac{a'''nf}{r^3} + \&c.} - \frac{S}{2ac} \times \overline{\frac{a'c}{r} + \frac{a''d}{r^2} + \frac{a'''e}{r^3} + \&c.}$
$- \frac{S}{2ac} \times \overline{\frac{a'd}{r} + \frac{a''e}{r^2} + \frac{a'''f}{r^3} + \&c.} - \frac{S}{2ab} \times \overline{\frac{a'b}{r} + \frac{a''m}{r^2} + \frac{a'''n}{r^3} + \&c.} -$
$_{2ab} \times \frac{a'm}{r} + \frac{a''n}{r^2} + \frac{a'''o}{r^3} + \&c. = G + M - \overline{D + E}.$

When the life of A is the oldeſt of the three lives, the ſymbols being changed as in the two preceding problems, the value of the reverſion will become $= - \frac{S}{6abc} \times \overline{\frac{abc'}{r} + \frac{msc''}{r^2} + \frac{ntc'''}{r^3} + \&c.} -$
$\frac{S}{2abcr} \times \overline{\frac{msc'}{r} + \frac{nt\,.\,\overline{c'+c''}}{r^2} + \&c.} + \frac{S}{6abc} \times \overline{\frac{bsc'}{r} + \frac{mtc''}{r^2} + \frac{nuc'''}{r^3} + \&c.} +$
$\frac{S}{2abcr} \times \frac{mt\,.\,c'}{r} + \frac{nu\,.\,\overline{c'+c''}}{r^2} + \&c. - \frac{S}{3abc} \times \frac{amc'}{r} + \frac{nsc''}{r^2} + \frac{otc'''}{r^3} + \&c. -$
$\frac{S}{2abcr} \times \overline{\frac{ns\,.\,c'}{r} + \frac{ot\,.\,\overline{c'+c''}}{r^2} + \&c.} + \frac{S}{3abc} \times \overline{\frac{msc'}{r} + \frac{nt\,.\,c''}{r^2} + \frac{ou\,.\,c'}{r^3} + \&c.} +$
$\frac{S}{2abcr} \times \overline{\frac{nt\,.\,c'}{r} + \frac{ou\,.\,\overline{c'+c''}}{r^2} + \&c.} + \frac{S}{2ac} \times \overline{\frac{\overline{a-s}\,.\,c'}{r} + \frac{\overline{s-t}\,.\,c''}{r^2} + \frac{\overline{t-u}\,.\,c'''}{r^3}}$
$\overline{+ \&c.} + \frac{S}{acr} \times \overline{\frac{\overline{s-t}\,.\,c'}{r} + \frac{\overline{t-u}\,.\,\overline{c'+c''}}{r^2} + \&c.}$

By the ſecond part of the ſolution of the firſt problem, the firſt eight ſeries may be found $= M - E$; and the laſt two ſeries being eaſily reſolved into $\frac{S}{a} \times \overline{\frac{\overline{a-s}}{r} + \frac{\overline{s-t}}{r^2} + \frac{\overline{t-u}}{r^3} + \&c.} -$
$\frac{S}{2ac} \times \overline{\frac{\overline{a-s}\,.\,\overline{c-c'}}{r} + \frac{\overline{s-t}\,.\,\overline{c-c'+c''}}{r^2} + \&c.} - \frac{S}{2ac} \times \overline{\frac{\overline{a-s}\,.\,c}{r} + \frac{\overline{s-t}\,.\,\overline{c-c'}}{r^2}}$
$\overline{+ \&c.}$ are $= G - D$. Hence the general rule, as in the former caſe, becomes $= G + M - \overline{D + E}$.

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When the lives are all equal, the above expreſſion will be changed into $\frac{S}{r} \times \overline{r-1} \times \overline{V-C+\frac{V-CCC}{3} - V-CC} = \frac{S.\overline{r-1}}{r} \times \frac{\overline{V-L}}{3}$; or "*one-third* the value of the abſolute reverſion after "the extinction of the longeſt of the three lives." This is known from ſelf-evident principles to be the true value, and therefore the foregoing ſolution is correct. The ſame rule may alſo be obtained immediately from the ſeries; for the value in this caſe for the firſt year will be $= \frac{S}{r} \times \overline{\frac{1}{3} - \frac{d}{c} + \frac{dd}{cc} - \frac{d^3}{3c^3}}$, for the ſecond year $= \frac{S}{r^2} \times \overline{\frac{d^3}{3c^3} - \frac{e^3}{3c^3} - \frac{dd}{cc} + \frac{d}{c} + \frac{ee}{cc} - \frac{e}{c}}$, and ſo on for the remaining years. Hence the whole value of the reverſion will be $= \frac{S}{3r} \times L + \frac{S}{3} \times \overline{\frac{1}{r} - L} = \frac{\overline{r-1}.S}{r} \times \frac{\overline{V-L}}{3}$ . . . . Q. E. D.

PROBLEM IV.

To determine the value of a given ſum payable on the extinction of the lives of A and B, ſhould they be the *firſt* that fail of the three lives A, B, and C.

SOLUTION.

The given ſum may be received in the firſt year, either on the extinction of the three lives, C having died laſt; or on the extinction of only the two lives A and B, C having ſurvived the year. In the ſecond and following years the given ſum may be received, provided either of ſix events ſhould happen. 1ſt, If the three lives ſhould become extinct in the year, C having been the laſt that failed. 2dly, If A only ſhould die in the year, B having died before the beginning, and C lived to the end of it. 3dly, If B only ſhould die in the year, A having died before the beginning, and C lived to the end of it. 4thly, If A and B ſhould both die in the year, and C ſurvive it. 5thly, If C ſhould die after A in the year, B having died in either of the foregoing years. 6thly, if C ſhould die after B in the year, A having died in either of the foregoing years. The fractions denoting theſe ſeveral contingencies being added together will be found =

$$\frac{S}{2ac}\times\overline{\frac{a'c}{r}+\frac{a''.d}{r^2}+\frac{a'''.e}{r^3}+\&c.}+\frac{S}{2ac}\times\overline{\frac{a\,d}{r}+\frac{a''e}{r^2}+\frac{a''.f}{r^3}+\&c.}-$$

$$\frac{S}{6abc}\times\overline{\frac{a'bc}{r}+\frac{a''md}{r^2}+\frac{a'''ne}{r^3}+\&c.}+\frac{S}{2abcr}\times\overline{\frac{ml.a'}{r}+\frac{ne.\overline{a'+a''}}{r^2}+\&c.}-$$

$$\frac{S}{3abc}\times\frac{a'mc}{r}+\frac{a''nd}{r^2}+\frac{a'''oe}{r^3}+\&c.-\frac{S}{2abcr}\times\frac{nd.a'}{r}+\frac{oe.\overline{a'+a''}}{r^2}+\&c.+$$

$$\frac{S}{6abc}\times\frac{a'bd}{r}+\frac{a''mc}{r^2}+\frac{a'''nf}{r^3}+\&c.+\frac{S}{2abcr}\times\frac{me.a'}{r}+\frac{nf.\overline{a'+a''}}{r^2}+\&c.-$$

$$\frac{2S}{3abc}\times\frac{a'md}{r}+\frac{a''ne}{r^2}+\frac{a'''of}{r^3}+\&c.-\frac{S}{2abcr}\times\frac{ne.a'}{r}+\frac{of.\overline{a'+a''}}{r^2}+\&c.$$

The firſt two ſeries are $= D$; the third and fourth ſeries are $= \frac{2}{3r}\times\overline{BC-ABC}-\frac{\beta\kappa.\overline{FK-AFK}}{6.bc}$; the fifth and ſixth ſeries are $= -\frac{\kappa.\overline{BK-ABK}}{3c}-\frac{m.\overline{PC-APC}}{6br}$; the ſeventh and eighth ſeries $= \frac{\beta.\overline{FC-AFC}}{6b}+\frac{d.\overline{BT-ABT}}{3cr}$; and the ninth and tenth ſeries $= \frac{md.\overline{PT-APT}}{6bcr}-\frac{2.\overline{BC-ABC}}{3}$. Hence the whole value of the reverſion will be

$$= D-\frac{S.\kappa}{3c}\times\overline{\frac{\beta.\overline{FK-AFK}}{2b}+\overline{BK-ABK}}+\frac{S.\beta}{6b}\times\overline{FC-AFC}-\frac{2.S.\overline{r-1}}{3r}\times\overline{BC-ABC}-\frac{S.m}{6br}\times\overline{PC-APC}+\frac{S.d}{3cr}\times\overline{BT-ABT}+\frac{m.\overline{PT-APT}}{2b}.$$

The preceding theorem expreſſes the value of the given ſum whether C be the oldeſt, or B one of the two lives to be ſurvived, and will therefore be ſufficient in all caſes. In order, however, ſtill more fully to prove this, let A be ſuppoſed the oldeſt life, and inſtead of $\overline{b-m}$, $\overline{m-n}$, $\overline{n-o}$, &c. and a', a'', a''', &c. let b', b'', b''', &c. and $\overline{a-s}$, $\overline{s-t}$, $\overline{t-u}$, &c. be ſubſtituted; then will the value of the reverſion be found =

$$\frac{S}{2bc}\times\overline{\frac{cb'}{r}+\frac{db''}{r^2}+\frac{eb'''}{r^3}+\&c.}+\frac{S}{2bc}\times\overline{\frac{ab'}{r}+\frac{eb''}{r^2}+\frac{fb'''}{r^3}+\&c.}-$$

$$\frac{S}{6abc}\times\overline{\frac{acb'}{r}+\frac{dsb''}{r^2}+\frac{etb'''}{r^3}+\&c.}+\frac{S}{2abcr}\times\overline{\frac{asb'}{r}+\frac{et\,.\,\overline{b'+b''}}{r^2}+\&c.}-$$

$$\frac{S}{3abc}\times\overline{\frac{csb'}{r}+\frac{dtb''}{r^2}+\frac{eub'''}{r^3}+\&c.}-\frac{S}{2abcr}\times\overline{\frac{dtb'}{r}+\frac{eu\,.\,\overline{b'+b''}}{r^2}+\&c.}+$$

$$\frac{S}{6abc}\times\overline{\frac{adb'}{r}+\frac{seb''}{r^2}+\frac{ftb'''}{r^3}+\&c.}+\frac{S}{2abcr}\times\overline{\frac{seb'}{r}+\frac{tf\,.\,\overline{b'+b''}}{r^2}+\&c.}-$$

$$\frac{2S}{3abc}\times\overline{\frac{dsb'}{r}+\frac{et\,.\,b''}{r^2}+\frac{fu\,.\,b'''}{r^3}+\&c.}-\frac{S}{2abcr}\times\overline{\frac{et\,.\,b'}{r}+\frac{fu\,.\,\overline{b'+b''}}{r^2}+\&c.}$$

Let Σ denote the value of S on the contingency of C's ſurviving B, and the general rule deduced from the preceding ſeries will become $=\Sigma-\frac{S\,.\,x}{3}\times\overline{\frac{\alpha\,.\,\overline{HK-HBK}}{2a}+\overline{AK-ABK}}+\frac{S\,.\,x}{6a}\times\overline{HC-HBC}-\frac{2S\,.\,\overline{r-1}}{3r}\times\overline{AC-ABC}-\frac{S\,.\,s}{6ar}\times\overline{NC-NBC}+\frac{S\,.\,d}{3cr}\times\overline{\overline{AT-ABT}+\frac{s\,.\,\overline{NT-NBT}}{2a}}$, which appears to be exactly the ſame rule with the foregoing, if the ſymbols of A and B be only exchanged for each other.

If the three lives be of the ſame age, both thoſe rules will ſeverally become = S into $\frac{\overline{r-1}}{2r}\times\overline{V-CC}-\frac{xx}{6cc}\times\overline{KK-CKK}-\frac{x}{6c}\times\overline{CK-CCK}-\frac{2\,.\,\overline{r-1}}{3r}\times\overline{CC-CCC}+\frac{d}{6cr}\times\overline{CT-CCT}+\frac{dd}{6ccr}\times$

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$\overline{TT-CTT}$. The two expreſſions $-\frac{xx\,.\,\overline{KK-CKK}}{6cc}+\frac{d}{6cr}\times\overline{CT-CCT}$, by reſolving them into their reſpective ſeries, will be found $=\frac{1-CC}{6r}+\overline{\frac{d}{6cr}+\frac{de}{6ccr^2}+\frac{ef}{6c^2r^3}+\frac{fg}{6c^2r^4}}$ + &c., and the two expreſſions $\frac{dd\,.\,\overline{TT-CTT}}{6ccr}-\frac{x}{c}\times\overline{CK-CCK}=\frac{CC}{6r}-\frac{d}{6cr}-\overline{\frac{de}{6ccr^2}+\frac{ef}{6ccr^3}+\frac{fg}{6ccr^4}}$ + &c.; hence the ſum of theſe four expreſſions will be $=-\frac{\overline{r-1}\,.\,\overline{V-CC}}{6r}$, and conſequently the general rule in this caſe will be $=\frac{S\,.\,\overline{r-1}}{3r}\times\overline{V-\overline{3CC-2CCC}}$, which is known to denote the true value from other principles*.

As a further proof of the accuracy of the preceding inveſtigation, it may not be improper to obſerve, that this rule may be immediately obtained from the different fractions which expreſs the value of the given ſum in each year. For in this caſe the value of S in the firſt year is $=\frac{S}{3}\times\overline{\frac{1}{r}-\frac{3dz}{ccr}+\frac{2d^3}{c^3\,.\,r}}$, in the ſecond year $=\frac{S}{3}\times\overline{\frac{2r^3}{c^3r^2}-\frac{3ce}{ccr^2}-\frac{2d^3}{c^3r^2}+\frac{3dd}{ccr^2}}$, and ſo on in the other years, which expreſſions may be eaſily found $=\frac{S\,.\,\overline{r-1}}{3r}\times\overline{V-\overline{3CC-2CCC}}$. Q. E. D.

PROBLEM V.

To find the value of a given ſum, payable on the death of A, if his life ſhould be the *firſt* or *ſecond* that fails of the three lives A, B, and C.

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* See my Paper in the Phil. Tranſ. Vol. LXXIX.

SOLU-

SOLUTION.

In the firſt year the payment of the given ſum depends upon either of four events. 1ſt, That all the three lives ſhall become extinct, the life of A having been the firſt or ſecond that failed. 2dly, That A and B ſhall both die, and C live to the end of the year. 3dly, That A and C ſhall both die, and B live to the end of the year. 4thly, That only A ſhall die, and B and C both live to the end of the year. In the ſecond and following years the payment of the given ſum will depend upon either of eight events happening. 1ſt, That all the three lives ſhall drop in the year, A having been the firſt or ſecond that failed. 2dly, That C ſurvives, and only A and B die in the year. 3dly, That B ſurvives, and only A and C die in the year. 4thly, That both B and C ſurvive, and A only dies in the year. 5thly, That A dies in the year, B having died before the beginning, and C lived to the end of it. 6thly, That A in like manner dies in the year, C having died before the beginning, and B lived to the end of it. 7thly, That C dies after A in the year, B's life having failed ineither of the preceding years. 8thly, That B dies after A in the year, C's life having failed in either of the preceding years. The fractions denoting theſe ſeveral contingencies are $= \frac{S}{2ac} \times \overline{\frac{a'c}{r} + \frac{a''\cdot d}{r^2} + \frac{a'''\cdot e}{r^3} + \&c.} + \frac{S}{2ac} \times \overline{\frac{a'd}{r} + \frac{a''\cdot e}{r^2} + \frac{a'''f}{r^3} + \&c.} + \frac{S}{2ab} \times \overline{\frac{a'b}{r} + \frac{a''\cdot m}{r^2} + \frac{a'''n}{r^3} + \&c.} + \frac{S}{2ab} \times \overline{\frac{a'm}{r} + \frac{a''n}{r^2} + \frac{a'''o}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{a'bc}{r} + \frac{a''md}{r^2} + \frac{a'''ne}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{a'mc}{r} + \frac{a''nd}{r^2} + \frac{a'''oe}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{a'bd}{r} + \frac{a''me}{r^2} + \frac{a'''nf}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{a'md}{r} + \frac{a''ne}{r^2} + \frac{a'''of}{r^3} + \&c.} = D + E - M.$

This general rule gives the true value whether the life of A be older or younger than both or either of the lives of B and C. When the three lives are of equal age, the value of S for the firſt year will be $= \frac{S}{r} \times \overline{\frac{2}{3} + \frac{a^3}{c^3} - \frac{dd}{cc}}$, for the ſecond year $= \frac{S}{r^2} \times \overline{\frac{e^3}{3c^3} - \frac{ee}{cc} - \frac{d^3}{3c^3} + \frac{dd}{cc}}$, and ſo on for the other years. Hence the whole value in this caſe will be $= \frac{S. \ -1}{3r} \times \overline{2V - \overline{3CC - CCC}}$, which expreſſion may alſo be derived from the general rule juſt given above, or $D + E - M$.

The ſolution of this problem may alſo be obtained either from the firſt and ſecond, or from the third problems. In the one caſe the value of S is evidently equal to the *ſum* of the two values determined by the two firſt-mentioned problems, or $D + E - 2M + M = D + E - M$. And in the other caſe its value is equal to the *difference* between the abſolute value of the reverſion after A $(= G)$ and its value depending upon the contingency that A ſhall be the *laſt* life that ſhall fail, which being $= G + M - \overline{D + E}$ by the third problem, it follows, that the general rule on this ſuppoſition will be alſo $= D + E - M$. Q. E. D.

PROBLEM VI.

To find the value of a given ſum payable on the death of A, ſhould his life be the *ſecond* or *third* that ſhall fail of the three lives A, B, and C.

SOLUTION.

The payment of the given ſum in the firſt year will depend upon the contingency of either of three events. 1ſt, That all the three lives ſhall become extinct, A having been the ſecond or third that has failed. 2dly, That A ſhall die after B, and C live to the end of the year. 3dly, That A ſhall die after C, and B live to the end of the year. In the ſecond and following years the given ſum will become payable, provided either of eight events ſhould happen. 1ſt, If all the three lives ſhould fail in the year, A having been the ſecond or third that died. 2dly, If A ſhould die after B in the year, and C live to the end of it. 3dly, If A ſhould die after C in the year, and B live to the end of it. 4thly, If A and C ſhould both die in the year, B having died before the beginning of it. 5thly, If A and B ſhould both die in the year, C having died before the beginning of it. 6thly, If A only ſhould die in the year, the lives of B and C having become extinct in either of the preceding years. 7thly, If A ſhould die in the year, B having died before the beginning, and C lived to the end of it. 8thly, if A ſhould die in the year, C having died before the beginning, and B lived to the end of it. Hence the whole value of the reverſion will be found =

$$\frac{S}{a} \times \overline{\frac{a'}{r} + \frac{a''}{r^2} + \frac{a'''}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{a'bc}{r} + \frac{a''md}{r^2} + \frac{a'''}{r^3}}$$

$$\frac{S}{6abc} \times \overline{\frac{a'mc}{r} + \frac{a''nd}{r^2} + \frac{a'''oe}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{a'bd}{r} + \frac{a''me}{r^2} + \frac{a'''nf}{r^3} + \&c.} -$$

$$\frac{S}{3abc} \times \overline{\frac{a'md}{r} + \frac{a''ne}{r^2} + \frac{a'''of}{r^3} + \&c.} = G - M.$$

This rule is correct in all caſes; but when the three lives are equal it becomes more ſimple, and is $= \frac{S \cdot \overline{r-1}}{3^r} \times \overline{2V - \overline{3C - CCC}}$; which expreſſion may likewiſe be obtained immediately from the ſeries given above.

The ſolution of this problem, like that of the foregoing one, may alſo be derived from the firſt three problems; for the value of S is either equal to the *difference* between the abſolute value of the reverſion after the death of A and its value depending on the contingency that A ſhall be the *firſt* that fails (found by Prob. 1.), or it is equal to the *ſum* of the two values depending on the contingencies that A ſhall be the *ſecond* or *third* that fails (found by Prob. 2. and 3.). In both caſes the general rule is = G – M. Q. E. D.

PROBLEM VII.

To find the value of a given ſum payable on the death of A, ſhould his life be the *firſt* or *laſt* that fails of the three lives A, B, C.

SOLUTION.

In order to receive the given ſum in the firſt year, it is neceſſary that either of four events ſhould happen. 1ſt, That all the three lives ſhould fail, A having been the firſt or third that died. 2dly, That B ſhould die after A, and C live. 3dly, That C ſhould die after A, and B live. 4thly, That A only ſhould die, and B and C both live. In the ſecond and following years the given ſum may be received, provided either of ſeven events ſhould happen. 1ſt, If the three lives ſhould fail, A having been the firſt or laſt that died. 2dly, if B ſhould die after A in the year, and C live to the end of it. 3dly, If C ſhould die after A in the year, and B live to the end of it. 4thly, If A only ſhould die in the year, and B and C both live to the end of it. 5thly, If A's life ſhould fail after that of B in the year, C's life having failed before the beginning of it. 6thly, If A ſhould fail after C in the year, B having failed before the beginning of it. 7thly, If A only ſhould die in the year, B and C having died in either of the preceding years. From the fractions denoting theſe ſeveral contingencies the whole value of the reverſion will be found =

$$\frac{S}{a} \times \overline{\frac{a'}{r} + \frac{a''}{r^2} + \frac{a'''}{r^3} + \&c.} - \frac{S}{2ac} \times \overline{\frac{a'c}{r} + \frac{a''d}{r^2} + \frac{a'''e}{r^3} + \&c.} -$$

$$\frac{S}{2ac} \times \overline{\frac{a'd}{r} + \frac{a''e}{r^2} + \frac{a'''f}{r^3} + \&c.} - \frac{S}{2ab} \times \overline{\frac{a'b}{r} + \frac{a''m}{r^2} + \frac{a'''n}{r^3} + \&c.} -$$

$$\frac{S}{2ab} \times \overline{\frac{a'm}{r} + \frac{a''n}{r^2} + \frac{a'''o}{r^3} + \&c.} + \frac{2S}{3abc} \times \overline{\frac{a'bc}{r} + \frac{a''md}{r^2} + \frac{a'''ne}{r^3} + \&c.} +$$

$$\frac{S}{3abc} \times \overline{\frac{a'mc}{r} + \frac{a''nd}{r^2} + \frac{a'''oe}{r^3} + \&c.} + \frac{S}{3abc} \times \overline{\frac{a'bd}{r} + \frac{a''me}{r^2} + \frac{a'''nf}{r^3} + \&c.} +$$

$$\frac{2S}{3abc} \times \overline{\frac{a'md}{r} + \frac{a''ne}{r^2} + \frac{a'''f}{r^3} + \&c.} = G - \overline{D + E} + 2M.$$

This general theorem will give the exact value in all caſes; but when the lives are equal, it is rendered more ſimple, by ſubſtituting the ſeveral values of G, D, E, and M, and will then become $= \frac{S \cdot \overline{r-1}}{3r} \times \overline{2V - 3C - 3CC + 2CCC}$; which expreſſion may alſo be derived in this particular caſe from the different ſeries denoting the value of S in each year.

The ſolution of this, like thoſe of the two preceding problems, may likewiſe be obtained by the aſſiſtance of the firſt three problems. For the value of this contingent reverſion is either equal to the *ſum* of the two values of S payable on the death of A, if his life ſhould be the firſt, or if it ſhould be the laſt that fails (found by Prob. 1. and 3.), or it is equal to the *difference* between the value of the abſolute reverſion after A's death, and the value of the contingent reverſion after A's death, provided he ſhould be the *ſecond* that fails of the three lives (found by Prob. 2.). In both caſes the general rule becomes $= G - \overline{D + E} + 2M$. Q. E. D.

PROBLEM VIII.

To find the value of a given ſum payable on the death of A or B, ſhould *either* of them be the *firſt* that ſhall fail of the three lives A, B, and C.

SOLUTION.

In each year the payment of the given ſum will depend upon either of ſix events. 1ſt, If the three lives ſhould fail in the year, A or B having died firſt. 2dly, if A and B ſhould die in the year, and A live. 3dly, If C ſhould die after A in the year, and B live. 4thly, If C ſhould die after B in the year, and A live. 5thly, If A only ſhould die in the year, and B and C both live. 6thly, If B only ſhould die in the year, and A and C both live. The fractions denoting theſe ſeveral contingencies are $= \frac{2S}{3abc} \times \overline{\frac{bca'}{r} + \frac{mc \cdot a''}{r^2} + \frac{nca'''}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{mca'}{r} + \frac{nca''}{r^2} + \frac{oea'''}{r^3} + \&c.} + \frac{S}{3abc} \times \overline{\frac{bda'}{r} + \frac{mea''}{r^2} + \frac{nf \cdot a''}{r^3} + \&c.} + \frac{S}{6abc} \times \overline{\frac{mda'}{a} + \frac{nea''}{r^2} + \frac{ofa'''}{r^3} + \&c.} + \frac{S}{2bc} \times \overline{\frac{\overline{b-m} \cdot c}{r} + \frac{\overline{m-n} \cdot d}{r^2} + \frac{\overline{n-o} \cdot e}{r^3} + \&c.} + \frac{S}{2bc} \times \overline{\frac{\overline{b-m} \cdot d}{r} + \frac{\overline{m-n} \cdot e}{r^2} + \frac{\overline{n-o} \cdot f}{r^3} + \&c.} - \frac{S}{2abc} \times \overline{\frac{bca'}{r} + \frac{md \cdot \overline{a'+a''}}{r^2} + \&c.} + \frac{S}{2abc} \times \overline{\frac{mca'}{r} + \frac{nd \cdot \overline{a'+a''}}{r^2} + \&c.} - \frac{S}{2abc} \times \overline{\frac{bda'}{r} + \frac{me \cdot \overline{a'+a''}}{r^2} + \&c.} + \frac{S}{2abc} \times \overline{\frac{md \cdot a'}{r} + \frac{ne \cdot \overline{a'+a''}}{r^2} + \&c.}$ = S into $\frac{n}{3c} \times \overline{\frac{\beta \cdot \overline{FK-AFK}}{2b} + \overline{BK-ABK} - \frac{\beta \cdot \overline{FC-AFC}}{6b} + \frac{2 \cdot \overline{r-1} \cdot \overline{BC-ABC}}{3r} + \frac{m \cdot \overline{PC-APC}}{6br}} - \frac{d}{3cr} \times \overline{\overline{BT-ABT} + \frac{m \cdot \overline{PT-APT}}{2b}} + \Sigma$ (Σ denoting the value of S on the contingency of C's ſurviving B, as in Prob. 4.)

The above rule gives the exact value when C is the oldeſt of the three lives. But if A be the oldeſt, the ſymbols muſt be changed as in ſome of the foregoing problems, and the value in this caſe will be expreſſed by the ſeries $\frac{2S}{3abc} \times \overline{\frac{abc'}{r} + \frac{msc''}{r^2} + \frac{ntc'''}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{amc'}{r} + \frac{nsc''}{r^2} + \frac{otc'''}{r^3} + \&c.} - \frac{S}{6abc} \times \overline{\frac{bsc'}{r} + \frac{mtc''}{r^2} + \frac{nuc'''}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{msc'}{r} + \frac{ntc''}{r^2} + \frac{ouc'''}{r^3} + \&c.} + \frac{S}{ab} \times \overline{\frac{ab}{r} + \frac{ms}{r^2} + \frac{nt}{r^3} + \&c.} - \frac{S}{ab} \times \overline{\frac{ms}{r} + \frac{nt}{r^2} + \frac{ou}{r^3} + \&c.} - \frac{S}{abc} \times \overline{\frac{abc'}{r} + \frac{ms\,.\,\overline{c'+c''}}{r^2} + \&c.} + \frac{S}{abc} \times \overline{\frac{msc'}{r} + \frac{nt\,.\,\overline{c'+c''}}{r^2} + \&c.}$ From theſe ſeveral ſeries, the general rule expreſſing the value of the reverſion will be found $=$ S into $\frac{\overline{r-1}\,.\,\overline{V-AB}}{r} - \frac{\alpha}{3a} \times \overline{\frac{\beta\,.\,\overline{HF-HFC}}{b} + \frac{HB-HBC}{2} - \frac{\beta\,.\,\overline{AF-AFC}}{6b} + \frac{2\,.\,\overline{r-1}\,.\,\overline{AB-ABC}}{3r}} + \frac{m\,.\,\overline{AP-APC}}{6br} + \frac{S}{3ar} \times \overline{\frac{BN-BNC}{2} + \frac{m\,.\,\overline{PN-PNC}}{b}}$.

When the lives are all equal, the firſt rule becomes $= \frac{\overline{r-1}\,.\,\overline{V-CC}}{2r} + \frac{2\,.\,\overline{r-1}\,.\,\overline{CC-CCC}}{3r} + \frac{xx\,.\,\overline{KK-CKK}}{6cc} + \frac{x\,.\,\overline{CK-CCK}}{6c} - \frac{d\,.\,\overline{CT-CCT}}{6cr} - \frac{dd\,.\,\overline{TT-CTT}}{6ccr}$, and the ſecond rule $= \frac{\overline{r-1}\,.\,\overline{V-CC}}{r} + \frac{2\,.\,\overline{r-1}\,.\,\overline{CC-CCC}}{3r} - \frac{xx\,.\,\overline{KK-CKK}}{3cc} - \frac{x\,.\,\overline{KC-CCK}}{3c} + \frac{d\,.\,\overline{CT-CCT}}{3cr} + \frac{dd\,.\,\overline{TT-CTT}}{3ccr}$. In the one caſe the four laſt fractions are $= \frac{\overline{r-1}\,.\,\overline{V-CC}}{6r}$; and in the other caſe thoſe fractions are $= -\frac{\overline{r-1}\,.\,\overline{V-CC}}{3r}$; therefore, in both caſes the general rule becomes $= \frac{2\,.\,S\,.\,\overline{r-1}}{3r} \times \overline{V-CCC}$, which is known to be the true value from ſelf-evident principles. This expreſſion may alſo be immediately derived from the different fractions which denote the value of S in each year. For in the firſt year theſe fractions will be reduced to $\frac{2S}{3}\times\overline{\frac{1}{r}-\frac{d^3}{c^3r}}$, in the ſecond year to $\frac{2S}{3}\times\overline{\frac{d^3-e^3}{c^3r^2}}$, in the third year to $\frac{2S}{3}\times\overline{\frac{e^3}{c^3r^3}-\frac{f^3}{c^3r^3}}$, and ſo on in the other years. Hence the whole value of the reverſion will, as above, be $=\frac{2\,.\,S\,.\,\overline{r-1}}{3r}\times\overline{V-CCC}$. Q. E. D *.

PROBLEM IX.

To determine the value of a given ſum payable on the death of A or B, ſhould *either* of them be the *ſecond* that fails of the three lives A, B, and C.

SOLUTION.

The payment of the given ſum in the firſt year will depend upon either of four events happening. 1ſt, That the three lives ſhould fail, A or B having been the ſecond that failed. 2dly, That A ſhould die after C, and B live. 3dly, That B ſhould die after C, and A live. 4thly, That A and B ſhould both die, and C live. In the ſecond and following years the given ſum will become payable, provided either of eleven events ſhould happen. 1ſt, If the three lives ſhould drop in the year, A or B having been the ſecond that failed. 2dly, If A ſhould die after C in the year, and B live. 3dly, If B ſhould die after C in the year, and A live. 4thly, If A and B

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[*] I do not know that any ſolution has been attempted before, either of this or of the two following problems.

ſhould both die in the year, and C live. 5thly, if B only ſhould die in the year, A having died before the beginning, and C lived to the end of it. 6thly, If A only ſhould die in the year, B having died before the beginning, and C lived to the end of it. 7thly, If C ſhould die after A in the year, B having died in either of the foregoing years. 8thly, If C ſhould die after B in the year, A having died in either of the foregoing years. 9thly, If A and B ſhould both die in the year, C having died in either of the preceding years. 10thly, If B only ſhould die in the year, C having died before the beginning, and A lived to the end of it. And, laſtly, if A only ſhould die in the year, C having died before the beginning, and B lived to the end of it. The ſeveral fractions denoting theſe contingencies in each year being added together will become

$$= \frac{S}{abc} \times \overline{\frac{bca'}{r} + \frac{md\,.\,\overline{a'+a''}}{r^2} + \&c.} - \frac{S}{abc} \times \overline{\frac{mca'}{r} + \frac{nd\,.\,\overline{a'+a'}}{r^2} + \&c.}$$

$$+ \frac{S}{abc} \times \overline{\frac{bda'}{r} + \frac{me\,.\,\overline{a'+a''}}{r^2} + \&c.} - \frac{S}{abc} \times \overline{\frac{m\ a'}{r} + \frac{ne\,.\,\overline{a'+a''}}{r} + \&c.} -$$

$$\frac{4S}{3abc} \times \overline{\frac{bca'}{r} + \frac{mda''}{r^2} + \frac{nea'''}{r^3} + \&c.} + \frac{S}{3abc} \times \overline{\frac{mca'}{r} + \frac{nda''}{r^2} + \frac{oea''}{r^3} + \&c.} -$$

$$\frac{2S}{3abc} \times \overline{\frac{bda'}{r} + \frac{mca''}{r^2} + \frac{nfa'''}{r^3} + \&c.} - \frac{S}{3abc} \times \overline{\frac{mda'}{r} + \frac{nea''}{r^2} + \frac{of\,.\,a'''}{r^3} + \&c.} -$$

$$\frac{S}{2bc} \times \overline{\frac{\overline{b-m}\,.\,c}{r} + \frac{\overline{m-n}\,.\,d}{r^2} + \frac{\overline{n-o}\,.\,e}{r^3} + \&c.} - \frac{S}{2bc} \times \overline{\frac{\overline{b-m}\,.\,d}{r} + \frac{\overline{m-n}\,.\,e}{r^2} + \frac{\overline{n-o}\,.\,f}{r^3}}$$

$$\overline{+ \&c.} + \frac{S}{ab} \times \overline{\frac{ma'}{r} + \frac{n\,.\,\overline{a'+a''}}{r^2} + \&c.} - \frac{S}{ab} \times \overline{\frac{ba'}{r} + \frac{m\,.\,\overline{a'+a''}}{r} + \&c.} +$$

$$\frac{S}{ab} \times \overline{\frac{ba'}{r} + \frac{ma''}{r^2} + \frac{na''}{r^3} + \&c.} + \frac{S}{b} \times \overline{\frac{\overline{b-m}\,.}{r} + \frac{\overline{m-n}\,.}{r^2} + \frac{\overline{n-o}}{r^3} + \&c.} +$$

$$\frac{S}{2ac} \times \overline{\frac{ca'}{r} + \frac{da''}{r^2} + \frac{ea'''}{r^3} + \&c.} + \frac{S}{2ac} \times \overline{\frac{da'}{r} + \frac{ea''}{r^2} + \frac{fa'''}{r^3} + \&c.}$$

Let Q denote the value of S by the firſt rule in the eighth problem, and the firſt ten ſeries will be $= \Sigma - 2Q$. The four next ſeries are$=\frac{S.\overline{r-1}.\overline{V-AB}}{r}$; and the two laſt ſeries are$=$D; the whole value of the reverſion, therefore (when C is the oldeſt of the three lives), is$=\frac{S.\overline{r-1}.\overline{V-A}}{r}+D+\Sigma-2Q$.

When A is the oldeſt of the three lives, the different ſeries, by changing the ſymbols, as in ſome of the foregoing problems, will become$=\frac{2S}{abc}\times\overline{\frac{abc'}{r}+\frac{ms.\overline{c'+c''}}{r}+\&c.}-\frac{2S}{abc}\times\overline{\frac{msc'}{r}+\frac{t.\overline{c'+c''}}{r}+\&c.}-$
$\frac{4S}{3abc}\times\overline{\frac{abc'}{r}+\frac{msc'}{r^2}+\frac{nt.c'''}{r^3}+\&c.}+\frac{2S}{3abc}\times\overline{\frac{ms.c'}{r}+\frac{nt.c'}{r^2}+\frac{ou.c'''}{r^3}+\&c.}$
$+\frac{S}{3abc}\times\overline{\frac{amc'}{r}+\frac{ntc''}{r}+\frac{oc'''}{r^3}+\&c.}+\frac{S}{3abc}\times\overline{\frac{bsc'}{r}+\frac{mtc''}{r^2}+\frac{nuc'''}{r^3}+\&c.}-$
$\frac{S}{bc}\times\overline{\frac{bc'}{r}+\frac{m.\overline{c'+c''}}{r^2}+\&c.}+\frac{S}{bc}\times\overline{\frac{mc'}{r}+\frac{n.\overline{c'+c''}}{r^2}+\&c.}+$
$\frac{S}{2bc}\times\overline{\frac{bc'}{r}+\frac{mc''}{r^2}+\frac{nc'''}{r^3}+\&c.}-\frac{S}{2bc}\times\overline{\frac{mc'}{r}+\frac{nc'}{r}+\frac{c''}{r^3}+\&c.}-$
$\frac{S}{ac}\times\overline{\frac{ac'}{r}+\frac{s.\overline{c'+c''}}{r^2}+\&c.}+\frac{S}{ac}\times\overline{\frac{sc'}{r}+\frac{t.\overline{c'+c''}}{r^2}+\&c.}+$
$\frac{S}{2ac}\times\overline{\frac{ac'}{r}+\frac{sc''}{r^2}+\frac{tc''}{r^3}+\&c.}-\frac{S}{2ac}\times\overline{\frac{sc'}{r}+\frac{tc''}{r}+\frac{uc'''}{r^3}+\&c.}+$
$\frac{S}{b}\times\overline{\frac{\overline{b-m}}{r}+\frac{\overline{m-n}}{r^2}+\frac{\overline{n-o}}{r^3}+\&c.}-\frac{S}{ab}\times\overline{\frac{\overline{b-m}.s}{r}+\frac{\overline{m-n}.t}{r^2}+\frac{\overline{n-o}.u}{r^3}+\&c.}$
$+\frac{S}{ab}\times\overline{\frac{\overline{b-m}.s}{r}+\frac{\overline{m-n}.t}{r^2}+\&c.}$ Let Q repreſent the value of S by the ſecond rule in the eighth problem, Π and Δ the values of the ſame ſum on the contingency of B's dying after C, and on the contingency of A's dying after C reſpectively [*]; then will the general rule in this caſe become$=\frac{S.\overline{r-1}.\overline{3V-B-A-AB}}{r}-2Q-\overline{\Pi+\Delta}$, or, becauſe $D+\Sigma$ are$=\frac{\overline{r-1}.\overline{2V-B-A}}{r}-\overline{\Pi+\Delta}$ [†], it

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[*] See my 3d Prob. in Phil. Tranſ. Vol. LXXVIII.

[†] See my 2d and 3d Prob. Vol. LXXVIII.

it will be $=\frac{S\,.\,\overline{r-1}\,.\,\overline{V-AB}}{r}+D+\Sigma-2Q$, as above.

When the lives are equal, the rule may be found either immediately from the ſeries, or from the foregoing expreſſions, $=\frac{2S\,.\,\overline{r-1}}{3r}\times\overline{V-3CC-2CCC}$. Q. E. D.

PROBLEM X.

To find the value of a given ſum payable on the deceaſe of B or C, ſhould *either* of them be the *laſt* that fails of the three lives A, B, and C.

SOLUTION.

The ſum S can be received in the firſt year only on the extinction of the three lives, reſtrained to the contingency of A's life having been the firſt or ſecond that failed. In the ſecond and following years it may be received provided either of ſix events ſhould happen. 1ſt, If the three lives ſhould fail in the year, A having been the firſt or ſecond that died. 2dly, If B and C ſhould both die in the year, A having died before the beginning of it. 3dly, If C only ſhould die in the year, A and B having died in either of the preceding years. 4thly, If B only ſhould die in the year, A and C having died in either of the preceding years. 5thly, If B ſhould die after A in the year, C having died before the beginning of it. 6thly, If C ſhould die after A in the year, B having died before the beginning of it. From the fractions expreſſing theſe ſeveral contingencies, the whole value of the reverſion will be found $=$

$$\frac{2S}{3abc}\times\overline{\frac{mda'}{r}+\frac{ne\,.\,\overline{a'+a'}}{r^2}+\&c.}-\frac{S}{3abc}\times\overline{\frac{bca'}{r}+\frac{md\,.\,\overline{a'+a''}}{r^2}+\&c.}-\frac{S}{r}$$

$$\frac{S}{6abc}\times\overline{\frac{ma}{r}+\frac{n\,a''}{r^2}+\frac{oea'''}{r^3}+\text{\&c.}}-\frac{S}{6abc}\times\overline{\frac{baa'}{r}+\frac{mea''}{r^2}+\frac{nf.a'''}{r^3}+\text{\&c.}}+$$
$$\frac{S}{2ab}\times\overline{\frac{ca'}{r}+\frac{m.\overline{a'+a''}}{r}+\frac{n.\overline{a'+a''+a'''}}{r^3}+\text{\&c.}}-\frac{S}{2ab}\times\overline{\frac{ma'}{r}+\frac{n.\overline{a'+a''}}{r^2}+}$$
$$\overline{\text{\&c.}}+\frac{S}{2ac}\times\overline{\frac{ca'}{r}+\frac{d.\overline{a'+a''}}{r^2}+\text{\&c.}}-\frac{S}{2ac}\times\overline{\frac{da'}{r}+\frac{e.\overline{a'+a''}}{r^2}+\text{\&c.}}-$$
$$\frac{2S}{3abcr}\times\overline{\frac{md.a'}{r}+\frac{ne.\overline{a'+a''}}{r^2}+\text{\&c.}}+\frac{S}{3abcr}\times\overline{\frac{ca'}{r}+\frac{of.\overline{a'+a''}}{r^2}+\text{\&c.}}+$$
$$\frac{S}{2abr}\times\overline{\frac{ma'}{r}+\frac{n.\overline{a'+a''}}{r^2}+\text{\&c.}}-\frac{S}{2abr}\times\overline{\frac{na'}{r}+\frac{o.\overline{a'+a''}}{r^2}+\text{\&c.}}+$$
$$\frac{S}{2acr}\times\overline{\frac{d.a'}{r}+\frac{e.\overline{a'+a''}}{r}+\text{\&c.}}-\frac{S}{2acr}\times\overline{\frac{ea'}{r}+\frac{f.\overline{a'+a''}}{r^2}+\text{\&c.}},$$

and therefore the general rule (when either B or C are the oldeſt of the three lives) will be $=$ S into $\frac{2.\overline{r-1}.\overline{BC-ABC}}{3r}-\frac{x}{3c}\times$ $\overline{\frac{\beta.\overline{FK-AFK}}{b}+\frac{\overline{BK-ABK}}{2}}\times\frac{m}{3br}\times\overline{\frac{\overline{PC-APC}}{2}+\frac{d.\overline{PT-APT}}{c}}-\frac{\beta.\overline{FC-AFC}}{6b}$ $+\frac{d.\overline{BT-ABT}}{6cr}+\frac{\beta.\overline{F-AF}}{2b}-\frac{\overline{r-1}.\overline{B-AB}}{2r}-\frac{m.\overline{P-AP}}{2br}+\frac{x.\overline{K-AK}}{2c}-$ $\frac{\overline{r-1}.\overline{C-AC}}{2r}-\frac{d.\overline{T-AT}}{2cr}$; or (ſince the firſt five expreſſions are $=$ $\frac{\overline{r-1}.\overline{BC-ABC}}{r}-M$, and the ſix remaining ones $=R+W$) it will be $=\frac{S.\overline{r-1}.\overline{BC-ABC}}{r}+R+W-M$.

When A *is the oldeſt of the three lives*, the ſame general rule may be obtained. In this caſe, by exchanging the ſymbols, the different ſeries will become $=\frac{S}{6abc}\times\overline{\frac{abc'}{r}+\frac{ms.\overline{c'+c''}}{r^2}+\text{\&c.}}-$ $\frac{S}{6abc}\times\overline{\frac{bs.c'}{r}+\frac{mt.\overline{c'+c''}}{r^2}+\text{\&c.}}+\frac{S}{3abc}\times\overline{\frac{amc'}{r}+\frac{ns.\overline{c'+c''}}{r^2}+\text{\&c.}}+$ $\frac{2S}{3abc}\times\overline{\frac{ms.c'}{r}+\frac{nt.\overline{c'+c''}}{r^2}+\text{\&c.}}+\frac{S}{c}\times\overline{\frac{c'}{r}+\frac{c''}{r^2}+\frac{c'''}{r^3}+\text{\&c.}}-$

$$\frac{S}{bc}\times\overline{\frac{mc'}{r}+\frac{n\,.\,\overline{c'+c''}}{r^2}+\&c.}-\frac{S}{2ac}\times\overline{\frac{ac'}{r}+\frac{sc''}{r^2}+\frac{tc'''}{r^3}+\&c.}-$$
$$\frac{S}{2ac}\times\overline{\frac{sc'}{r}+\frac{t\,.\,c''}{r^2}+\frac{uc'''}{r^3}+\&c.}-\frac{2S}{3abcr}\times\overline{\frac{ms\,.\,c'}{r}+\frac{nt\,.\,\overline{c+c''}}{r^2}+\&c.}-$$
$$\frac{S}{3abcr}\times\overline{\frac{mt\,.\,c'}{r}+\frac{nu\,.\,\overline{c'+c''}}{r^2}+\&c.}+\frac{S}{6abcr}\times\overline{\frac{rsc'}{r}+\frac{ot\,.\,\overline{c'+c''}}{r^2}+\&c.}-$$
$$\frac{S}{6abcr}\times\overline{\frac{nt\,.\,c'}{r}+\frac{ou\,.\,\overline{c'+c''}}{r^2}+\&c.}+\frac{S}{bcr}\times\overline{\frac{mc'}{r}+\frac{n\,.\,\overline{c'+c''}}{r^2}+\&c.}=\text{S into}$$
$$\frac{2\,.\,\overline{r-1}\,.\,\overline{AB-ABC}}{3r}+\frac{\beta}{6b}\times\overline{\frac{\alpha\,.\,\overline{HF-HFC}}{a}-\overline{AF-AFC}}+\frac{\alpha\,.\,\overline{HB-HBC}}{3a}$$
$$-\frac{s}{3ar}\times\overline{NB-NBC+\frac{m\,.\,\overline{PN-PNC}}{2b}}+\frac{m\,.\,\overline{AP-APC}}{6br}+\frac{\overline{r-1}\,.\,\overline{V-C}}{r}-$$
$$\frac{\overline{r-1}\,.\,\overline{B-BC}}{r}-\frac{\overline{r-1}\,.\,\overline{A-AC}}{2r}-\frac{\alpha\,.\,\overline{H-HC}}{2a}+\frac{s\,.\,\overline{N-NC}}{2ar}.$$

In order to get the ſame general rule as that given above in the caſe of B or C's being the oldeſt of the three lives, it is to be obſerved, that the firſt five expreſſions, by the ſecond part of the ſolution of Prob. 1. appear to be $=\frac{\overline{r-1}\,.\,\overline{AB-ABC}}{r}-M+E$. And (ſuppoſing Γ to repreſent the value of S on the contingency of A's dying after C) that the laſt three expreſſions appear to be $=-\Gamma-\frac{\overline{r-1}\,.\,\overline{A-AC}}{r}$. But E is $=R+\frac{\overline{r-1}\,.\,\overline{B-AB}}{r}$, and W muſt be $=\frac{\overline{r-1}\,.\,\overline{V-A-C+AC}}{r}-\Gamma$; therefore, the ſum of the above expreſſions may be eaſily found $=\frac{S\,.\,\overline{r-1}\,.\,\overline{BC-ABC}}{r}+R+W-M$.

If the three lives be of equal age, the value of the reverſion will be $=\frac{2S\,.\,\overline{r-1}}{3r}\times\overline{V-L}$. This expreſſion may be derived either from the foregoing general rule, or immediately from the different ſeries, and is known to be accurately true from ſelf-evident principles. Q. E. D.

I have now given general rules for determining the values of reverſions depending upon three lives in every caſe which, as far as I can diſcover, will admit of an *exact* ſolution. The remaining caſes, which are nearly equal in number to thoſe I have inveſtigated, involve a contingency for which it appears very difficult to find ſuch a general expreſſion as ſhall not render the rules much too complicated and laborious. The contingency to which I refer is that of *one life's failing after another in any given time.*. The fractions expreſſing this probability are every year increaſing, ſo that the value of the reverſion muſt be repreſented by as many ſeries at leaſt as are equal to the difference between the age of one of the lives, and that of the oldeſt life in the table of obſervations. I have indeed ſo far ſucceeded in the method of approximation as that the reverſion may be generally aſcertained within about $\frac{1}{50}$th part of its exact value; but I ſhall not trouble the Royal Society at preſent with theſe inveſtigations.

The 34th, 35th, and 36th problems in Mr. Simpson's Select Exerciſes involve this contingency, and, by the aſſiſtance of M. de Moivre's hypotheſis, admit of an eaſy ſolution. But ſuch is the fallacy of this hypotheſis, that it renders Mr. Simpson's concluſions obviouſly wrong, though his reaſoning is perfectly correct: for it cannot ſurely be an *equal* chance *in all caſes* that one life ſhall die after another. In the ſhort term of a ſingle year the chances are indeed ſo nearly equal, that it would be wrong to perplex the ſolution by attempting greater accuracy. But when the number of years, and the difference between the ages of the two lives are, conſiderable, thoſe chances muſt vary in proportion; and, therefore, unleſs the contingency is blended with another which ſhall very much diminiſh the probability of the event, the ſolution, by thus indiſcriminately ſuppoſing the chances to be equal, muſt be rendered extremely inaccurate. In Mr. SIMPSON's 36th problem the ſolution by this means appears to be abſurd: for, in the particular caſe in which C is the oldeſt of the three lives, the value of the reverſionary annuity becomes $=\frac{C-AC}{2}$; that is, the value of an annuity in this caſe during the life of C after B and A, provided A dies firſt, is the ſame whatever be the age of B; for no mention is made of his life in the foregoing expreſſion. It ſhould be obſerved, however, that the rule itſelf is ſtrictly true, and that the error ariſes from Mr. SIMPSON's having been miſled by the hypotheſis in determining the probability of B's dying after A in his inveſtigation of the 34th problem, which is applied to the ſolution of this problem *.

I have declined giving ſpecimens of the different values of the reverſions as deduced from the foregoing rules and thoſe which have been hitherto publiſhed, not only from an apprehenſion of becoming tedious, but alſo from the conviction that at preſent they are unneceſſary; thoſe which I have formerly given being, I think, ſufficient to prove the inaccuracy of M. DE MOIVRE's hypotheſis. In thoſe inſtances in which I have compared ſome of the foregoing rules with the approxi-

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* It is proper to obſerve, that I have followed Mr. SIMPSON's method of determining this contingency in the 23d, 27th, 28th, and 29th Problems in my Treatiſe on Annuities.

mations now in uſe, I have invariably found the latter to be erroneous; nay, in ſome caſes, the values were almoſt twice as great as they ought to have been. This is particularly true when one of the lives is very young, and both or either of the other lives are very old. In reverſions of this kind I believe that this is generally the caſe, and that it ſeldom happens that the ages of the three lives are nearly equal. The approximations therefore can hardly ever be uſed with ſafety, and it will certainly be moſt prudent not to have recourſe to them when the correct values can be obtained. Should the difficulties attending the ſolution of the remaining problems which involve three lives be ſurmounted (and the taſk may not perhaps be impoſſible), the hypotheſis of an equal decrement of life, as far as it relates to any uſeful purpoſe in the doctrine of annuities, may then be totally abandoned. Or ſhould it even be found impracticable to deduce ſolutions of thoſe problems which are ſtrictly and accurately true; yet, I am ſatisfied from my own experience that ſuch near approximations may be procured as to render this hypotheſis equally unneceſſary.

XVIII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland; *by* Thomas Barker, *Eſq.; with the Rain in* Surrey *and* Hampſhire; *for the Year* 1790. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 2, 1791.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.				Surrey.	Hampſhire.	
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	S. Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,97	28,48	29,55	49	36	41	50½	26½	37	1,871	1,49	1,99	1,72
	Aftern.				49	36	41½	52½	34½	41				
Feb.	Morn.	30,05	29,20	29,71	48	40	43½	48	30	38½	0,236	0,20	0,49	0,43
	Aftern.				49½	41	44½	57	40½	47				
Mar.	Morn.	30,13	29,31	29,77	50½	42½	46	48½	31	39½	0,259	0,24	0,45	0,38
	Aftern.				51	44	47	60	42½	50				
Apr.	Morn.	29,85	29,01	29,42	51	40	44	49½	32	38½	0,676	2,54	3,64	1,27
	Aftern.				52½	41	45	61	38	49				
May	Morn.	29,80	28,93	29,45	58	50½	54	57	43½	49½	2,911	3,70	4,38	3,66
	Aftern.				59½	51	55	72	52½	61				
June	Morn.	29,86	28,98	29,56	71	54½	59	65½	48½	55½	2,385	0,64	0,13	0,55
	Aftern.				75½	55½	60½	85	56	67½				
July	Morn.	29,73	29,90	29,35	64	57	60	62	49½	57	2,246	2,42	3,24	1,71
	Aftern.				66	58	61	78½	62	69				
Aug.	Morn.	29,68	29,17	29,49	66	56	60½	62½	49	56	1,735	2,26	2,30	1,97
	Aftern.				67½	57	62	80	57	69				
Sept.	Morn.	29,95	28,88	29,53	62	52	55½	59½	42	49½	1,566	0,52	0,66	0,62
	Aftern.				61	53	57	72½	55½	61				
Oct.	Morn.	29,81	28,89	29,43	57	45½	52	55	36	45½	0,991	1,72	2,10	1,25
	Aftern.				59	45½	53	65	46	55				
Nov.	Morn	29,88	28,49	29,35	49½	37½	44	48	30	40	3,145	3,40	6,95	5,11
	Aftern.				49½	38	45	51½	32	44½				
Dec.	Morn.	29,87	28,32	29,33	46	36	40	48	25½	38½	3,608	3,18	5,94	3,38
	Aftern.				47	36	40½	49	31	43				
										Inches	21,629	22,31	32,27	22,05

The thermometer abroad being broken in December, that month is defective in that article.

THE winter proved a remarkably open one; no froſt of two whole days together, nor any ſnow; ſo that there were young artichokes in the garden all winter, where they were ſhort and among the leaves; and the ſecond crop of figs, which are generally killed in winter, eſcaped this year, and ripened in the ſummer, but did not grow large. It was ſaid there were young hares in winter, and birds' eggs laid, ſome of which were perhaps hatched. Several people ſaid they ſaw a covey of young partridges near Oakham in February; but I cannot affirm it, for others ſuſpected ſome miſtake. January was ſhowery and mild, but often windy; as the days grew longer, it became dryer, finer, and forward. A ſwarm of bees roſe and ſettled at Tixover, March 2.; but it was rather a ſhifting of their hive for want, than a regular ſwarm, which could not have been bred ſo early. The owner, having a deſerted hive with combs and honey in it, put them into that, where they lived, and grew ſtrong in the ſummer. The forwardneſs of the ſeaſon continued till the N.E. winds of March made it colder and more froſty; the growth of all things was then ſtopped, and the firſt fortnight in April was ſeverer than any part of the winter, with the only ſnow this ſeaſon.

The ſeed time was dry, and in general good; but, for want of froſt in the winter, the ground ploughed ſtiff, and required a great deal of working; and the ſeaſon continued dry and backward till after the middle of May, when rain and warmer weather made every thing grow as faſt as they were ſlow before. The ſummer was chiefly cloudy and windy, and after the middle of May very ſhowery and cool, except ſome few violent hot days; and a very growing ſeaſon, ſo that grounds laid at the end of May had good crops of hay on them, which in general was well got, for the rains in this country were not great, though frequent; and ſo much land was laid late, and ſo much over-eaten graſs cut, that there was hardly ever more good hay made than there was this year. The harveſt alſo was ſhowery, yet not ſo much but that thoſe who were not too haſty got it well, and it was plentiful, though very rank, and pretty much laid, yielding well to the acre, though not to the flail.

The autumn was dryer, fine, and pleaſant, ſo that the ground began to burn. The wheat ſeed time was remarkably good; for it was dry at firſt for ſowing the clay land; then rain came in good time to bring it up, and that made the ground moiſt for ſowing the dry fields. There was a good deal of rain at times in November, yet ſo much fine weather between, that it was pleaſant and not dirty; but moſt part of December and January was uncommonly ſtormy and uncertain weather, changing ſometimes two or three times a day; ſevere ſtorms, much rain, great floods, and remarkable thunder, on ſeveral different days, and in many different places. Near a fortnight's froſt at Chriſtmas, with clear weather at firſt, but afterward one great rain, and ſeveral leſſer, which yet did not take away the froſt till January the 6th.

Chalk found in a new place.

There is a great deal of chalky ground in the ſouthern part of England; I think it begins at the ſea in Devonſhire, and one vein of it runs all along the ſouthern counties to Dover. Another vein parts off from that about Reading in Berkſhire, goes by Dunſtable, Baldock, and Gogmagog Hills, and ſo on to the ſea in Norfolk; the whole croſſing the kingdom in a Y. Along theſe two diſtricts it is almoſt all chalk to a great depth in the ground; but out of them chalk is ſeldom found. I believe it may be met with in many places in the countries between theſe two diſtricts, and ſometimes deep in the ground, where it does not come up to the ſurface; but beyond the northern limits of them, which are at Wantage in Berkſhire, and over the river from Shillingford in Oxfordſhire, and at Maddingley by Cambridge, chalk is hardly any where to be found; no where in any conſiderable quantity, unleſs it be much farther north, in the wolds of Yorkſhire, beyond Pocklington toward Scarborough.

I did not know till lately that we had any chalk nearer us than Maddingley; but ſeveral years ago, the people of Ridlington in Rutland, digging for ſtone to mend the roads, met with a bed of chalk; at which they were much ſurprized, and did not know what it was, having never ſeen a chalk pit before. After I had heard of it, I went to examine the place, and found it a regular chalk pit, with rows of flints lying in it as is uſual in the ſouth of England. The chalk is not ſoft like that they write with, but very much like that they dig about Baldock; nor are the flints ſo black as thoſe in the ſouth of England, but veined, of a light-coloured flint, and white, ſome parts much mixed with chalk; and are broken, not whole ones. They may have dug the pit ſix yards long and two deep; but how far the chalk reaches I do not know. The ground about it has plainly been formerly dug, perhaps thirty yards ſquare, but completely turfed over again, with the ſame ſtrong turf as the reſt of the cloſe, which is rich paſture land, and feeds oxen for Smithfield market, not like the ſhort graſs on the chalky downs.

Riding laſt autumn along the turnpike road near Stukeley in Huntingdonſhire, I ſaw a little patch of chalk, a few yards long, in a bank which had been dug away by the road ſide; ſo that though we did not know there was any chalk at all in this country, and there certainly is very little, yet here are now two places where it has been met with.

XIX. *Deſcription of a ſimple Micrometer for meaſuring ſmall Angles with the Teleſcope. By Mr.* Tiberius Cavallo, *F. R. S.*

Read June 2, 1791.

THE various teleſcopical micrometers, or machines which have been conſtructed for the meaſurement of ſmall angles, may be divided into two claſſes; namely, thoſe which have not, and thoſe which have, ſome movement amongſt their parts. The micrometers of the former ſort conſiſt moſtly of fine wires, or hairs, variouſly diſpoſed, and ſituated within the teleſcope, juſt where the image of the object is formed. In order to determine an angle with thoſe micrometers, a good deal of calculation is generally required. The micrometers of the other ſort, of which there is a great variety; ſome being made with moveable parallel wires, others with priſms, others again with a combination of lenſes, and ſo on; are more or leſs ſubject to ſeveral inconveniences, the principal of which are the following. 1ſt, Their motions generally depend upon the action of a ſcrew, and of courſe the imperfections of its threads, and the greater or leſs quantity of loſt motion, which is obſervable in moving a ſcrew, eſpecially when ſmall, occaſion a conſiderable error in the menſuration of angles. 2dly, Their complication and bulk renders them difficultly applicable to a variety of teleſcopes, eſpecially to the pocket ones. 3dly, They do not meaſure the angle without ſome loſs of time, which is neceſſary to turn the ſcrew, or to move ſome other mechaniſm. 4thly, and laſtly, They are conſiderably expenſive, ſo that ſome of them coſt even more than a tolerably good teleſcope.

After having had long in view the conſtruction of a micrometer, which might be in part at leaſt, if not intirely, free from all thoſe objections; and, after various attempts, I at laſt ſucceeded with a ſimple contrivance, which, after repeated trials, has been found to anſwer the deſired end, not only from my own experience, but from that alſo of ſeveral friends, to whom it has been communicated.

This micrometer, in ſhort, conſiſts of a thin and narrow ſlip of mother of pearl finely divided, and ſituated in the focus of the eye-glaſs of a teleſcope, juſt where the image of the object is formed. It is immaterial whether the teleſcope be a refractor or a reflector, provided the eye-glaſs be a convex lens, and not a concave one, as in the Galilean conſtruction.

The ſimpleſt way of fixing it is to ſtick it upon the diaphragm, which generally ſtands within the tube, and in the focus of the eye-glaſs. When thus fixed, if you look through the eye-glaſs, the diviſions of the micrometrical ſcale will appear very diſtinct, unleſs the diaphragm is not exactly in the focus; in which caſe the micrometrical ſcale muſt be placed exactly in the focus of the eye-glaſs, either by puſhing the diaphragm backwards or forwards, when that is practicable; or elſe the ſcale may be eaſily removed from one or the other ſurface of the diaphragm by the interpoſition of a circular piece of paper or card, or by a bit of wax. This conſtruction is fully ſufficient when the teleſcope is always to be uſed by the ſame perſon; but when different perſons are to uſe it, then

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the diaphragm, which ſupports the micrometer, muſt be conſtructed ſo as to be eaſily moved backwards or forwards, though that motion needs not be greater than about a tenth or an eighth of an inch. This is neceſſary, becauſe the diſtance o the focus of the ſame lens appears different to the eyes of different perſons, and therefore, whoever is going to uſe the teleſcope for the menſuration of any angle, muſt firſt of all unſcrew the tube, which contains the eye-glaſs and micrometer, from the reſt of the teleſcope, and, looking through the eye-glaſs, muſt place the micrometer where the diviſions of it may appear quite diſtinct to his eye.

In caſe that any perſon ſhould not like to ſee always the micrometer in the field of the teleſcope, then the micrometrical ſcale, inſtead of being fixed to the diaphragm, may be fitted to a circular perforated plate of braſs, wood, or even paper, which may be occaſionally placed upon the ſaid diaphragm.

I have made ſeveral experiments to determine the moſt uſeful ſubſtance for this micrometer. Glaſs, which I had ſucceſsfully applied for a ſimilar purpoſe to the compound microſcope, ſeemed at firſt to be the moſt promiſing; but it was at laſt rejected after ſeveral trials: for the diviſions upon it generally are either too fine to be perceived, or too rough; and though with proper care and attention the diviſions may be proportioned to the ſight, yet the thickneſs of the glaſs itſelf obſtructs in ſome meaſure the diſtinct view of the object. Ivory, horn, and wood, were found uſeleſs for the conſtruction of this micrometer, on account of their bending, ſwelling, and contracting very eaſily; whereas mother of pearl is a very ſteady ſubſtance, the diviſions upon it may be marked very eaſily, and, when it is made as thin as common writing paper, it has a very uſeful degree of tranſparency.

Tab. VIII. fig. 1. exhibits this micrometer ſcale, but ſhews it four times larger than the real ſize of one, which I have adapted to a three-feet achromatic teleſcope, that magnifies about 84 times. It is ſomething leſs than the 24th part of an inch broad; its thickneſs is equal to that of common writing paper; and the length of it is determined by the aperture of the diaphragm, which limits the field of the teleſcope. The diviſions upon it are the 200dths of an inch, which reach from one edge of the ſcale to about the middle of it, excepting every fifth and tenth diviſion, which are longer. The divided edge of it paſſes through the center of the field of view, though this is not a neceſſary precaution in the conſtruction of this micrometer. Two diviſions of the above deſcribed ſcale in my teleſcope are very nearly equal to one minute; and as a quarter of one of thoſe diviſions may be very well diſtinguiſhed by eſtimation, therefore an angle of one-eighth part of a minute, or of $7''\frac{1}{2}$, may be meaſured with it.

When a teleſcope magnifies more, the diviſions of the micrometer muſt be more minute; and I find, that when the focus of the eye-glaſs of the teleſcope is ſhorter than half an inch, the micrometer may be divided with the 500dths of an inch; by means of which, and the teleſcope magnifying about 200 times, one may eaſily and accurately meaſure an angle ſmaller than half a ſecond.

On the other hand, when the teleſcope does not magnify above 30 times, the diviſions need not be ſo minute: for inſtance, in one of DOLLOND's pocket teleſcopes, which when drawn out for uſe, is about 14 inches long, a micrometer with the hundredths of an inch is quite ſufficient, and one of its diviſions is equal to little leſs than three minutes, ſo that an angle of a minute may be meaſured by it.

In looking through a teleſcope, furniſhed with ſuch a micrometer, the field of view appears divided by the micrometer ſcale, the breadth of which occupies about one-ſeventh part of the aperture, and as the ſcale is ſemitranſparent, that part of the object, which happens to be behind it, may be diſcerned ſufficiently well to aſcertain the diviſion, and even the quarter of a diviſion, with which its borders coincide. Fig. 2. ſhews the appearance of the field of my teleſcope with the micrometer, when directed to the title page of the Philoſophical Tranſactions, wherein one may obſerve that the thickneſs of the letter C is equal to three-fourths of a diviſion, the diameter of the O is equal to three diviſions, and ſo on.

At firſt view one is apt to imagine that it is difficult to count the diviſions which may happen to cover or to meaſure an object; but upon trial it will be found, that this is readily performed; and even people, who have never been uſed to obſerve with the teleſcope, ſoon learn to meaſure very quickly and accurately with this micrometer; for, ſince every fifth and tenth diviſion is longer than the reſt, one ſoon acquires the habit of ſaying five, ten, fifteen, and then, by adding the other diviſions leſs than five, completes the reckoning. Even with a teleſcope, which has no ſtand, if the object end of it be reſted againſt a ſteady place, and the other end be held by the hand near the eye of the obſerver, an object may be meaſured with accuracy ſufficient for ſeveral purpoſes, as for the eſtimation of ſmall diſtances, for determining the height of a houſe, &c.

After having conſtructed and adapted this micrometer to the teleſcope, it is then neceſſary to aſcertain the value of the diviſions. It is hardly neceſſary to mention in this place, that though thoſe diviſions meaſure the chords of the angles, and not the angles or arches themſelves, and the chords are not as the arches, yet it has been ſhewn by all the trigonometrical writers, that in ſmall angles the chords, arches, ſines, and tangents, follow the ſame proportion ſo very nearly, that the very minute difference may be ſafely neglected: ſo that if one diviſion of this micrometer is equal to one minute, we may ſafely conclude, that two diviſions are equal to two minutes, three diviſions to three minutes, and ſo on. There are various methods of aſcertaining the value of the diviſions of ſuch a micrometer, they being the very ſame that are uſed for aſcertaining the value of the diviſions in other micrometers. Such are the paſſage of an equatorial ſtar over a certain number of diviſions in a certain time, the meaſuring of the diameter of the ſun, by computation from the focal diſtance of the object, and other lenſes of the teleſcope, the laſt of which, however, is ſubject to ſeveral inaccuracies; but as they are well known to aſtronomical perſons, and have been deſcribed in many books, need not be farther noticed in this Paper. However, for the ſake of workmen and other perſons not converſant in aſtronomy, I ſhall deſcribe an eaſy and accurate method of aſcertaining the value of the diviſions of the micrometer.

Mark upon a wall, or other place, the length of ſix inches, which may be done by making two dots or lines ſix inches aſunder, or by fixing a ſix-inch ruler upon a ſtand; then place the teleſcope before it ſo that the ruler or ſix-inch length may be at right angles with the direction of the teleſcope, and juſt 57 feet 3½ inches diſtant from the object-glaſs of the teleſcope: this done, look through the teleſcope at the ruler or other extenſion of ſix inches, and obſerve how many diviſions of the micrometer are equal to it, and that ſame number of diviſions is equal to half a degree, or 30′; and this is all that needs be done for the required determination; the reaſon of which is, becauſe an extenſion of ſix inches ſubtends an angle of 30′ at the diſtance of 57 feet 3½ inches, as may be eaſily calculated by the rules of plane trigonometry.

In one of DOLLOND's 14-inch pocket teleſcopes, if the diviſions of the micrometer be the hundredths of an inch, 11½ of thoſe diviſions will be found equal to 30′, or 23 to a degree.

When this value has been once aſcertained, any other angle meaſured by any other number of diviſions is determined by the rule of three. Thus, ſuppoſe that the diameter of the ſun, ſeen through the ſame teleſcope, be found equal to 12 diviſions, ſay as 11½ diviſions are to 30 minutes, ſo are 12 diviſions to $\left(\frac{12' \times 30'}{11,5}\right)$ 31′,3, which is the required diameter of the ſun.

Notwithſtanding the facility of this calculation, a ſcale may be made anſwering to the diviſions of a micrometer, which will ſhew the angle correſponding to any number of diviſions to mere inſpection. Thus, for the above-mentioned ſmall teleſcope the ſcale is repreſented in fig. 3. AB is a line drawn at pleaſure; it is then divided into 23 equal parts, and thoſe diviſions, which repreſent the diviſions of the micrometer that are equal to one degree, are marked on one ſide of it. The line then is divided again into 60 equal parts, which are marked on the other ſide of it; and theſe diviſions repreſent the minutes which correſpond to the diviſions of the micrometer: thus the figure ſhews, that ſix diviſions of the micrometer are equal to 15½ minutes, 11¼ diviſions are nearly equal to 29 minutes, &c. What has been ſaid of minutes may be ſaid of ſeconds alſo, when the ſcale is to be applied to a large teleſcope.

Thus far this micrometer, and its general uſe, have been ſufficiently deſcribed, and mathematical perſons may eaſily apply it to the various purpoſes to which micrometers have been found ſubſervient. But as the ſimplicity, cheapneſs, and at the ſame time the accuracy of this contrivance, may render the uſe of it much more general than that of any other micrometer; and I may venture to ſay, that it will be found very uſeful in the army, and amongſt ſea-faring people, for the determination of diſtances, heights, &c.; I ſhall therefore ſubjoin ſome practical rules to render this micrometer uſeful to perſons unacquainted with trigonometry and the uſe of logarithms.

Problem I. The angle, not exceeding one degree, which is ſubtended by an extenſion of one foot being given, to find its diſtance from the place of obſervation.

N. B. This extenſion of one foot, or any other which may be mentioned hereafter, muſt be perpendicular to the direction of the teleſcope through which it is obſerved. The diſtances are reckoned from the object-glaſs of the teleſcope, and the anſwers obtained by the rules of this problem, though not exactly true, are however ſo little different from the truth, that the difference ſeldom amounts to more than two or three inches, which may be ſafely neglected.

Rule 1. If the angle be expreſſed in minutes, ſay, as the given angle is to 60, ſo is 687,55 to a fourth proportional, which gives the anſwer in inches.

2. If the angle be expreſſed in ſeconds, ſay, as the given angle is to 3600, ſo is 687,55 to a fourth proportional, which expreſſes the anſwer in inches.

3. If the angle be expreſſed in minutes and ſeconds, turn it all into ſeconds, and proceed as above.

Example. At what diſtance is a globe of one foot in diameter when it ſubtends an angle of two ſeconds?

$2 : 3600 :: 687{,}55 : \frac{3600 \times 687{,}55}{2} = 1237590$ inches, or $103132\frac{1}{2}$ feet, which is the anſwer required.

This calculation may be ſhortened; for ſince two of the three proportionals are fixed, their product in the firſt caſe is 41253, and in the other two caſes is 2475180; ſo that in the firſt caſe, *viz.* when the angle is expreſſed in minutes, you need only divide 41253 by the given angle; and in the other two caſes, *viz.* when the angle is expreſſed in ſeconds, divide 2475180 by the given angle, and the quotient in either caſe is the anſwer in inches.

Problem II. The angle, not exceeding one degree, which is ſubtended by any known extenſion, being given, to find its diſtance from the place of obſervation.

Rule. Proceed as if the extenſion were of one foot by Problem I. and call the anſwer B; then, if the extenſion in queſtion be expreſſed in inches, ſay, as 12 inches are to that extenſion, ſo is B to a fourth proportional, which is the anſwer in inches; but if the extenſion in queſtion be expreſſed in feet, then you need only multiply it by B, and the product is the anſwer in inches.

Example. At what diſtance is a man, ſix feet high, when he appears to ſubtend an angle of 30″.

By Problem I. if the man were one foot high, the diſtance would be 82506 inches; but as he is ſix feet high, therefore multiply 82506 by 6, and the product gives the required diſtance, which is 495036 inches, or 41253 feet.

For greater conveniency, eſpecially in travelling, or in ſuch circumſtances in which one has not the opportunity of making even the eaſy calculations required in thoſe problems, I have calculated the following two tables; the firſt of which ſhews the diſtance anſwering to any angle from one minute to one degree, which is ſubtended by an extenſion of one foot; and the ſecond table ſhews the diſtance anſwering to any angle from one minute to one degree, which is ſubtended by a man, the height of which has been called an extenſion of ſix feet; becauſe, at a mean, ſuch is the height of a man when dreſſed with hat and ſhoes on. Theſe tables may be tranſcribed on a card, and may be had always ready with a pocket teleſcope furniſhed with a micrometer. Their uſe is evidently to aſcertain diſtances without any calculation; and they are calculated only to minutes, becauſe with a pocket teleſcope and micrometer it is not poſſible to meaſure an angle more accurately than to a minute.

Thus, if one wants to meaſure the extenſion of a ſtreet, let a foot ruler be placed at the end of the ſtreet; meaſure the angular appearance of it, which ſuppoſe to be 36′, and in the table you will have the required diſtance againſt 36′, which is $95\frac{1}{2}$ feet. Thus alſo a man, who appears to be 49′ high, is at the diſtance of 421 feet.

T. CAVALLO.

Wells-ſtreet,
May 26, 1791.

Angles ſubtended by an extenſion of one foot at different diſtances.

Angles.	Diſtances in feet.	Angles.	Diſtances in feet.
Min. 1	3437,7	Min. 31	110.9
2	1718,9	32	107,4
3	1145,9	33	104,2
4	859,4	34	101,1
5	687,5	35	98,2
6	572.9	36	95,5
7	491,1	37	92,9
8	429,7	38	90,4
9	382,0	39	88,1
10	343,7	40	85,9
11	312,5	41	83,8
12	286,5	42	81,8
13	264,4	43	79,9
14	245,5	44	78,1
15	229,2	45	76,4
16	214,8	46	74.7
17	202,2	47	73,1
18	191,0	48	71,6
19	180,9	49	70,1
20	171,8	50	68,7
21	162,7	51	67,4
22	156,2	52	66,1
23	149,4	53	64,8
24	143,2	54	63,6
25	137,5	55	62,5
26	132,2	56	61,4
27	127,3	57	60,3
28	122,7	58	59,2
29	118,5	59	58,2
30	114,6	60	57,3

Angles ſubtended by an extenſion of ſix feet at different diſtances.

Angles.	Diſtances in feet.	Angles.	Diſtances in feet.
Min. 1	20626,8	Min. 31	665,4
2	10313.	32	644,5
3	6875,4	33	625.
4	5156,5	34	606,6
5	4125,2	35	589,3
6	3437,7	36	572,9
7	2946,6	37	557,5
8	2578,2	38	542,8
9	2291,8	39	528,9
10	2062,6	40	515,6
11	1875,2	41	503,1
12	1718,8	42	491,1
13	1586,7	43	479,7
14	1473,3	44	468,8
15	1375.	45	458,4
16	1289,1	46	448,4
17	1213,3	47	438,9
18	1145,9	48	429,7
19	1085,6	49	421.
20	1031,4	50	412,5
21	982,1	51	404,4
22	937,6	52	396,7
23	896,8	53	389,2
24	859,4	54	381,9
25	825.	55	375.
26	793,3	56	368,3
27	763,9	57	361,9
28	736,6	58	355,6
29	711,3	59	349,6
30	687,5	60	343,7

❧

Fig. 2.

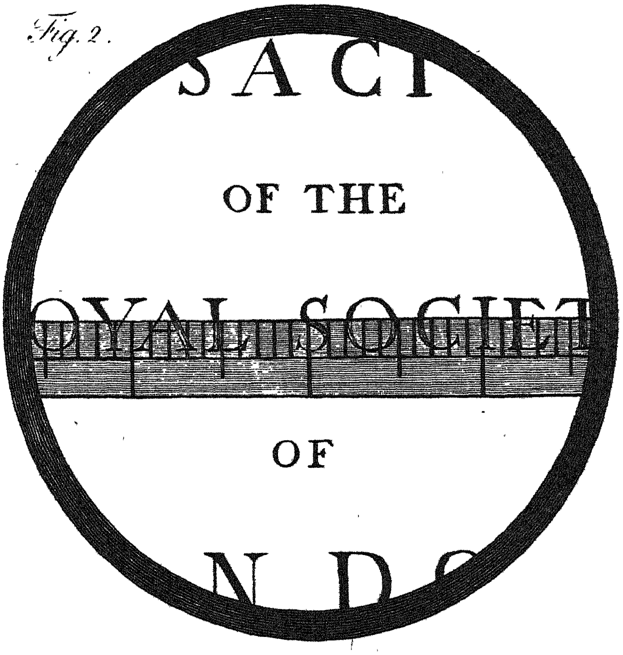


Fig. 1.

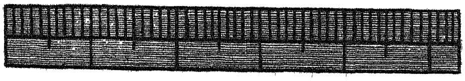
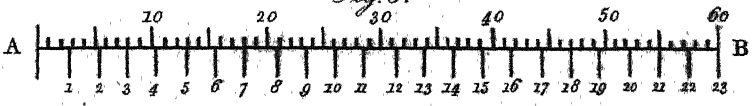


Fig. 3.



XX. *A new Method of inveſtigating the Sums of Infinite Series.*

By the Rev. Samuel Vince, *A. M. F. R. S.*

Read June 2, 1791.

THE ſummation of infinite ſeries is a ſubject, not only of curious ſpeculation, but alſo of the greateſt importance in the various branches of mathematics and philoſophy; in conſequence of which it has always claimed a very conſiderable ſhare of attention from the moſt celebrated mathematicians. I ſhall therefore make no apology for offering to the public the following new and very expeditious method, by which we may obtain the ſums of a great variety of ſeries, moſt of which have never before been treated of. As the ſummation depends on the ſums of the reciprocals of the powers of the natural numbers, tables of ſuch ſums are given as far as the 40th power to twelve places of decimals, by which the ſums of the ſeries will be found true to ten or eleven places; and if greater accuracy were required (which is a caſe that can very rarely happen) it might eaſily be obtained by continuing the tables. The firſt and third columns ſhew the ſums, and the ſecond and fourth the powers correſponding.

TABLE I.

Sum of $\frac{1}{2^n} + \frac{1}{3^n} + \frac{1}{4^n} +$ &c. *ad infinitum.*

Sum	$n =$	Sum	$n =$
A = ,644934066848	2	W = ,000000238450	22
B = ,202056903159	3	X = ,000000119219	23
C = ,082323233711	4	Y = ,000000059608	24
D = ,036927755107	5	Z = ,000000029803	25
E = ,017343061984	6	A′ = ,000000014901	26
F = ,008349277387	7	B′ = ,000000007450	27
G = ,004077356198	8	C′ = ,000000003725	28
H = ,002008392826	9	D′ = ,000000001863	29
I = ,000994575128	10	E′ = ,000000000931	30
K = ,000494188604	11	F′ = ,000000000465	31
L = ,000246086553	12	G′ = ,000000000233	32
M = ,000122713347	13	H′ = ,000000000116	33
N = ,000061248135	14	I′ = ,000000000058	34
O = ,000030588236	15	K′ = ,000000000029	35
P = ,000015282259	16	L′ = ,000000000015	36
Q = ,000007637196	17	M′ = ,000000000007	37
R = ,000003817292	18	N′ = ,000000000004	38
S = ,000001908212	19	O′ = ,000000000002	39
T = ,000000953961	20	P′ = ,000000000001	40
V = ,000000476932	21		

TABLE II.

Sum of $\frac{1}{2^n} - \frac{1}{3^n} + \frac{1}{4^n} - \frac{1}{5^n}$ + &c. *ad infinitum.*

Sum	$n =$	Sum	$n =$
a = ,177532966576	2	*w* = ,000000238386	22
b = ,098457322630	3	*x* = ,000000119199	23
c = ,052967170503	4	*y* = ,000000059602	24
d = ,027880229587	5	*z* = ,000000029801	25
e = ,014448908703	6	*a′* = ,000000014901	26
f = ,007406180072	7	*b′* = ,000000007450	27
g = ,003766998147	8	*c′* = ,000000003725	28
h = ,001905702459	9	*d′* = ,000000001863	29
i = ,000960492403	10	*e′* = ,000000000931	30
k = ,000482856502	11	*f′* = ,000000000465	31
l = ,000242314856	12	*g′* = ,000000000233	32
m = ,000121457237	13	*h′* = ,000000000116	33
n = ,000060829654	14	*i′* = ,000000000058	34
o = ,000030448787	15	*k′* = ,000000000029	35
p = ,000015235790	16	*l′* = ,000000000015	36
q = ,000007621708	17	*m′* = ,000000000007	37
r = ,000003812130	18	*n′* = ,000000000004	38
s = ,000001906491	19	*o′* = ,000000000002	39
t = ,000000953389	20	*p′* = ,000000000001	40
v = ,000000476742	21		

TABLE III.

Sum of $\frac{1}{2^n}+\frac{1}{4^n}+\frac{1}{6^n}+$ &c. *ad infinitum.*

Sum	n =	Sum	n =
A″ = ,411233516712	2	W‴ = ,000000238419	22
B″ = ,150257112895	3	X″ = ,000000119209	23
C″ = ,067645202107	4	Y″ = ,000000059605	24
D″ = ,032403992347	5	Z″ = ,000000029802	25
E″ = ,015895985344	6	A‴ = ,000000014901	26
F″ = ,007877728730	7	B‴ = ,000000007450	27
G″ = ,003922177173	8	C‴ = ,000000003725	28
H″ = ,001957047643	9	D‴ = ,000000001863	29
I″ = ,000977533765	10	E‴ = ,000000000931	30
K″ = ,000488522553	11	F‴ = ,000000000465	31
L″ = ,000244200705	12	G‴ = ,000000000233	32
M″ = ,000122085292	13	H‴ = ,000000000116	33
N″ = ,000061038895	14	I‴ = ,000000000058	34
O″ = ,000030518512	15	K‴ = ,000000000029	35
P″ = ,000015259024	16	L‴ = ,000000000015	36
Q″ = ,000007629452	17	M‴ = ,000000000007	37
R″ = ,000003814712	18	N‴ = ,000000000004	38
S″ = ,00000 907352	19	O‴ = ,000000000002	39
T″ = ,000000953675	20	P‴ = ,000000000001	40
V″ = ,000000476837	21		

TABLE

TABLE IV.

Sum of $\frac{1}{3^n}+\frac{1}{5^n}+\frac{1}{7^n}+$ &c. *ad infinitum*

Sum	$n=$	Sum	$n=$
a'' = ,233700550136	2	n'' = ,000000209240	14
b'' = ,051799790264	3	o'' = ,000000069724	15
c'' = ,014678031684	4	p'' = ,000000023234	16
d'' = ,004523762760	5	q'' = ,000000007744	17
e'' = ,001447076640	6	r'' = ,000000002581	18
f'' = ,000471548657	7	s'' = ,000000000864	19
g'' = ,000155179025	8	t'' = ,000000000286	20
h'' = ,000051345183	9	v'' = ,000000000095	21
i'' = ,000017041362	10	w'' = ,000000000032	22
k'' = ,000005666051	11	x'' = ,000000000011	23
l'' = ,000001885848	12	y'' = ,000000000004	24
m'' = ,000000628055	13	z'' = ,000000000001	25

PROP. I.

To find the ſum of the ſums of the reciprocal ſquares, cubes, &c. &c. *ad infinitum.*

By diviſion $\frac{1}{\overline{x-1}\times x}=\frac{1}{x^2}+\frac{1}{x^3}+\frac{1}{x^4}+$ &c. *ad inf*; hence if we make each of theſe terms the general term of a ſeries, and write 2, 3, 4, &c. *ad inf.* for x, we have $\frac{1}{1\cdot2}+\frac{1}{2\cdot3}+\frac{1}{3\cdot4}+$ &c. = (Table 1.) A + B + C + D + &c.; but $\frac{1}{1\cdot2}+\frac{1}{2\cdot3}+\frac{1}{3\cdot4}+$ &c. *ad inf.* = 1; hence A + B + C + D + &c. *ad inf.* = 1.

As $\frac{1}{x\times\overline{x+1}}=\frac{1}{x^2}-\frac{1}{x^3}+\frac{1}{x^4}-\frac{1}{x^5}+$&c. *ad inf.*; we have, by the ſame method of proceeding, A − B + C − D + &c. *ad inf.* = ½; conſequently A + C + E + &c. = ¾, and B + D + F + &c. = ¼.

Becauſe $\frac{1}{\overline{x-1}\times x}=\frac{1}{x^2}+\frac{1}{x^3}+\frac{1}{x^4}+$&c. *ad inf.*; if for x we write 2, 4, 6, &c., then will $\frac{1}{1\cdot 2}+\frac{1}{3\cdot 4}+\frac{1}{5\cdot 6}+$&c. = (Tab. 3) $A''+B''+C''+D''+$&c.; but $\frac{1}{1\cdot 2}+\frac{1}{3\cdot 4}+\frac{1}{5\cdot 6}+$&c. = hyp. log. 2; hence $A''+B''+C''+D''+$&c. = hyp. log. 2.

If in the ſame expreſſion we write 3, 5, 7, &c. for x, then $\frac{1}{2\cdot 3}+\frac{1}{4\cdot 5}+\frac{1}{6\cdot 7}+$ &c. = (Tab. 4.) $a''+b''+c''+$&c.; but $\frac{1}{2\cdot 3}+\frac{1}{4\cdot 5}+\frac{1}{6\cdot 7}+$&c. = 1 − hyp. log. 2; hence $a''+b''+c''$ + &c. = 1 − hyp. log. 2.—Hence from either of theſe two laſt caſes, we have a very expeditious method of finding the hyp. log. 2.

PROP. II.

To find the ſum of the infinite ſeries whoſe general term is

$mx^r\pm n$

By diviſion $\frac{1}{mx^r\pm n}=\frac{1}{mx^r}\mp\frac{n}{m^2x^{2r}}+\frac{n^2}{m^3x^{3r}}\mp\frac{n^3}{m^4x^{4r}}+$ &c. *ad inf.*; hence, if $\frac{1}{mx^r\pm n}$ be made the general term of a ſeries, and for x we write 2, 3, 4, &c., its ſum will be equal to the ſums of another ſet of ſerieſes, whoſe terms are the powers of the reciprocals of the natural numbers reſpectively multiplied into $\frac{1}{m}$, $\frac{n}{m^2}$, $\frac{n^2}{m^3}$, &c.; hence the ſum of each of theſe ſeries being known from the tables, the ſum of the given ſeries will be found.

Ex. 1. Let $\frac{1}{x^2+1}$ be the general term; now $\frac{1}{x^2+1}=\frac{1}{x^2}-\frac{1}{x^4}+\frac{1}{x^6}-\frac{1}{x^8}+$&c.; hence if for x we write 2, 3, 4, &c. we have $\frac{1}{5}+\frac{1}{10}+\frac{1}{17}+\frac{1}{26}+$&c. $=A-C+E-G+$ &c. = (by Tab. 1.) ,576674037469.

Ex. 2. Let $\frac{1}{x^2-1}$ be the general term; then, by the ſame method of proceeding, $\frac{1}{3}+\frac{1}{8}+\frac{1}{15}+\frac{1}{24}+$ &c. $=A+C+E+$ &c. = (by Prop. 1.) $\frac{3}{4}$.

Cor. Becauſe $\frac{1}{8}+\frac{1}{24}+\frac{1}{48}+$&c. $=\frac{1}{8}\times\overline{1+\frac{1}{3}+\frac{1}{6}+\&c.}$ = (as $1+\frac{1}{3}+\frac{1}{6}+$&c. is the reciprocal of the figurative numbers of the ſecond order) $\frac{1}{8}\times 2=\frac{1}{4}$; therefore $\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+$ &c. $=\frac{1}{2}$. Alſo, as $\frac{1}{x-1}=\frac{1}{x^2}+\frac{1}{x^4}+\frac{1}{x^6}+$&c.; if we write 2, 4, 6, &c. for x, we have $\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+$&c. = (by Tab. 3.) $A''+C''+E''+$ &c. $=\frac{1}{2}$; but, by Prop. 1. $A''+B''+C''+D''+$&c. = hyp. log. 2; hence $B''+D''+F''+$&c. $=-\frac{1}{2}+$hyp. log. 2.

Ex. 3. Let the general term be $\frac{1}{x^3-1}=\frac{1}{x^3}+\frac{1}{x^6}+\frac{1}{x^9}+$&c., and, by writing 2, 3, 4, &c. for x, we have $\frac{1}{7}+\frac{1}{26}+\frac{1}{63}+$ &c. $=B+E+H+$&c. = ,221689395104.

Ex. 4. Let the general term be $\frac{1}{3x^4-2}=\frac{1}{3x^4}+\frac{2}{9x}+\frac{4}{27x^{12}}+$ &c., and, by writing 2, 3, 4, &c. for x, &c. we have $\frac{1}{46}+\frac{1}{241}+\frac{1}{766}+$ &c. $\frac{1}{3}\text{C}+\frac{2}{9}\text{G}+\frac{4}{27}\text{L}+$ &c. $=$,02838525052.

Ex. 5. To find the ſum of the ſeries $\frac{1}{9}-\frac{1}{26}+\frac{1}{65}-\frac{1}{124}+$ &c. If we write 2, -3, 4, -5, &c. for x, the general term will be $\frac{1}{x^3+1}=\frac{1}{x^3}-\frac{1}{x^6}+\frac{1}{x^9}-\frac{1}{x^{12}}+$ &c. Now, by writing 2, -3, 4, -5, &c. for x, the ſerieſes of which $\frac{1}{x^3}$, $\frac{1}{x^9}$, &c. are the general terms, will be alternately $+$ and $-$, and therefore their ſums will be found in Tab. 2. and the ſerieſes of which $\frac{1}{x}$, $\frac{1}{x^2}$, &c. are the general terms will have their terms all $+$, and therefore their ſums will be found in Tab. 1. Hence the ſum required $= b+h+o+$ &c. $-\text{E}-\text{L}-\text{R}-$ &c. $=$,08-800,3.803.

PROP. III.

To find the ſum of the ſums of the reciprocals of the odd *powers in* Tab. 2.

*

By diviſion $\frac{1}{\overline{x-1}\times x}=\frac{1}{x^2}+\frac{1}{x^3}+\frac{1}{x^4}+\frac{1}{x^5}+$ &c.; hence by writing 2, -3, 4, -5, &c. for x, the ſums of the ſerieſes of which $\frac{1}{x^3}$, $\frac{1}{x}$, &c are the general terms, may be found by Tab. 2. and the other ſums by Tab. 1.; hence $\frac{1}{1\cdot2}+\frac{1}{3\cdot4}+\frac{1}{5\cdot6}+$ &c. $=\text{A}+\text{C}+\text{E}+$ &c. $+b+d+f+$ &c.; but $\frac{1}{1\cdot2}+\frac{1}{3\cdot4}+\frac{1}{5\cdot6}+$ &

&c. $= -\frac{1}{2} + 2$ hyp. log. 2; and by Prop. 1. $A + C + E +$ &c. $=$ $\frac{3}{4}$; hence $b+d+f+$&c. $= -\frac{5}{4} + 2$ hyp. log. 2.

PROP. IV.

To find the ſum of the infinite ſeries whoſe general term is $\frac{x^s}{mx^r \pm n}$.

By diviſion $\frac{x^s}{mx^r \pm n} = \frac{1}{mx^{r-s}} \mp \frac{n}{m^2x^{2r-s}} + \frac{n^2}{m^3x^{3r-s}} \mp$ &c. *ad inf.*; hence the ſum of the ſeries of which $\frac{x^s}{mx^r \pm n}$ is the general term, is found as in Prop. 2. Here r muſt be greater than s at leaſt by 2, otherwiſe the ſum will be infinite.

Ex. 1. Let the general term be $\frac{x^2}{x^4+1} = \frac{1}{x^2} - \frac{1}{x^6} + \frac{1}{x^{10}} -$ &c.; hence if for x we write 2, 3, 4, &c. we have $\frac{4}{17} + \frac{9}{82} + \frac{16}{257} +$ &c. $= A - E + I - N +$ &c. $= ,538527924723$.—If for x we write 2, 4, 6, &c. we get $\frac{4}{17} + \frac{16}{257} + \frac{36}{1296} +$ &c. $= A'' - E'' + I'' - N'' +$ &c. $= ,396257616555$.

Ex. 2. Let the general term be $\frac{x}{3x^3-1} = \frac{1}{3x^2} + \frac{1}{9x^5} + \frac{1}{27x^8} +$ &c.; hence if we write 2, 3, 4, &c. for x, we have $\frac{2}{23} + \frac{3}{80} + \frac{4}{191} +$ &c. $= \frac{1}{3}A + \frac{1}{9}D + \frac{1}{27}G +$ &c. $= ,219238483448$.

By this propoſition we may find the ſum of any ſeries whoſe general term is $\frac{ax^s + bx^{s-1} + cx^{s-2} + \text{\&c.}}{mx^r \pm n}$; for this reſolves itſelf into $\frac{ax^s}{mx^r \pm n}$, $\frac{bx^{s-1}}{mx^r \pm n}$, &c. &c., the ſum of each of which ſeries is found by this propoſition. Now the $\overline{s+1}$th differences of the numerators of this general term are $=0$, and therefore it comprehends all ſeries under ſuch circumſtances. For example, let the given ſeries be $\frac{4}{17}+\frac{13}{82}+\frac{26}{257}+\frac{43}{626}$. Here the third differences of the numerators $=0$; to find therefore the general expreſſion for the numerator, aſſume ax^2+bx+c for it; and, by writing 2, 3, 4, for x, we have $4a+2b+c=4$, $9a+3b+$ $-c=13$, $16a+4b+c=26$; hence $a=2$, $b=-1$, $c=-2$; and as the denominator is manifeſtly x^4+1, the general term will be $\frac{2x^2-x-2}{x^4+1}=\frac{2x^2}{x^4+1}-\frac{x}{x^4+1}-\frac{2}{x^4+1}$, each of which being made the general term of a ſeries, their ſum will be found to be reſpectively 1,077055849446, 0,194173022145, and 0,156955159332; hence the ſum of the given ſeries is 0,725927667969.

If s be negative, the general term becomes— $\frac{1}{x^s\times\overline{mx^r\pm n}}-\frac{1}{mx^{r+s}}$

$\mp\frac{1}{m^2x^{2r+s}}+\frac{1}{m^3x^{3r+s}}\mp$ &c.

Ex. 1. To find the ſum of $\frac{1}{1.2.3}-\frac{1}{2.3.4}+\frac{1}{3.4.5}-$ &c. *ad inf.* Here the general term is $\frac{1}{\overline{x-1}\times x\times\overline{x+1}}=\frac{1}{x\times\overline{x^2}}$. $\frac{1}{x^3}+\frac{1}{x^5}+\frac{1}{x^7}+$&c.; hence, by writing 2, -3, 4, -5, &c. for x, we have the ſum $=b+d+f+$ &c. $=$ (by Prop. 3.) $-\frac{5}{4}+2$ hyp. log. 2.

If $\frac{1}{\overline{x-1}\times x^3\times\overline{x+1}}$ be the general term it reſolves itſelf into $\frac{1}{x^5}+\frac{1}{x^7}+\frac{1}{x^7}+$&c.; conſequently the ſum of $\frac{1}{1.2^3.3}-\frac{1}{2.3^3.4}$ +

$+\frac{1}{3 \cdot 4 \cdot 5} - \&c. = -b - \frac{5}{4} + 2$ hyp. log. 2. In like manner the ſum of $\frac{1}{1 \cdot 2^5 \cdot 3} - \frac{1}{2 \cdot 3^5 \cdot 4} + \frac{1}{3 \cdot 4^5 \cdot 5} - \&c. = -b - d - \frac{5}{4} + 2$ hyp. log. 2. Thus we may proceed as far as we pleaſe by adding two powers to the middle term; and hence this remarkable property of the ſerieſes, that the difference of the ſums of the ſerieſes where the middle term is x, x^3, x^5, &c. is b, d, f, &c. reſpectively.

Ex. 2. In like manner if the general term be $\frac{1}{\overline{x-1} \times x^3 \times \overline{x+1}}$, and we write 2, 3, 4, &c. for x, we have $\frac{1}{1 \cdot 2^3 \cdot 3} + \frac{1}{2 \cdot 3^3 \cdot 3} + \frac{1}{3 \cdot 4^3 \cdot 5} + \&c. = D + F + H + \&c. =$ (by Prop. 1.) $\frac{1}{4} - B$. Hence alſo $\frac{1}{1 \cdot 2^5 \cdot 3} + \frac{1}{2 \cdot 3^5 \cdot 4} + \&c. = \frac{1}{4} - B - D$; and ſo on as before.

If the general term be under the form $\frac{1}{x^n \cdot \overline{x+m}}$, it will be moſt convenient to reſolve it thus: by diviſion $\frac{1}{x+m} = \frac{1}{x} - \frac{m}{x^2} + \frac{m^2}{x^3} - \&c. \pm \frac{m^n}{x^n \cdot \overline{x+m}}$; hence $\pm \frac{1}{x^n \cdot \overline{x+m}} = \overline{\frac{1}{x+m} - \frac{1}{x} + \frac{m}{x^2} - \frac{m^2}{x^3} + \&c.} \times \frac{1}{m^n} = \overline{-\frac{m}{x \cdot \overline{x+m}} + \frac{m}{x^2} - \frac{m^2}{x^3} + \&c.} \times \frac{1}{m^n}$, where the ſign on the left hand will be + or − according as n is even or odd, and the number of terms on the right is $= n$. Now the ſum of the ſeries whoſe general term is $\frac{m}{x \cdot \overline{x+m}}$ is well known, and the ſums of the other are found from the tables.

Ex. 1. To find the ſum of $\frac{1}{2^2 \cdot 3} + \frac{1}{3^2 \cdot 4} + \frac{1}{4^2 \cdot 5} + \&c.$ *ad inf.*

Here the general term is $\frac{1}{x^2 \times \overline{x+1}} = -\frac{1}{x.\overline{x+1}} + \frac{1}{x^2}$, and by writing 2, 3. 4, &c. for x, we have the ſum $= -\frac{1}{2.3} - \frac{1}{3.4} - \&c. + A = -\frac{1}{2} + A$. In like manner $\frac{1}{2^3.3} + \frac{1}{4^2.5} + \frac{1}{6.7} + \&c. = -1 + \text{hyp.log.}\,2 + A''$. Alſo $\frac{1}{2^3.5} + \frac{1}{3^3.6} + \frac{1}{4^3.7} + \&c. = \overline{\frac{13}{12} - 3A + 9B} \times \frac{1}{27}$.

If m be negative, then $\frac{1}{x^n.\overline{x-m}} = \overline{\frac{m}{x.\overline{x-m}} - \frac{m}{x^2} - \frac{m^2}{x^3} - \&c.} \times \frac{1}{m^n}$. Hence $\frac{1}{2^4.1} + \frac{1}{3^4.2} + \frac{1}{4^4.3} + \&c. = 1 - A - B - C$; and ſo on for others of the ſame kind.

If the general term be under this form $\frac{1}{x^{rn}.\overline{ax^n+m}}$, then, in like manner, we have $\pm \frac{1}{x^{rn}.\overline{ax^n+m}} = \overline{\frac{1}{ax^n+m} - \frac{1}{ax^n} + \frac{m}{a^2x^{2n}} - \&c.} \times \frac{a^r}{m^r}$, where the ſign on the left hand will be + or −, according as r is even or odd, and the number of terms on the right is $= r+1$.

Ex. 1. To find the ſum of $\frac{1}{2^4.5} + \frac{1}{3^4.10} + \frac{1}{4^4.17} + \&c.$ Here $m = 1$, $n = 2$, $r = 2$, $a = 1$, and the general term $\frac{1}{x^4 \times \overline{x^2+1}} = \frac{1}{x^2+1} - \frac{1}{x^2} + \frac{1}{x^4}$; now the ſum of the ſeries whoſe general term is $\frac{1}{x^2+1}$ is $=,576674037469$, by Prop. 2.; conſequently the ſum required $=,576674037469 - A + C = ,014063204332$.

Ex. 2. If the given ſeries be $\frac{1}{4.5} + \frac{1}{9.10} + \frac{1}{16.17} + \&c.$ the general term will be $\frac{1}{x^{r}\cdot\overline{x+1}} = -\frac{1}{x^{r}+1} + \frac{1}{x}$; hence, by writing 2, 3, 4, &c. for x, we have the ſum $= -,576674037469 + A = ,06826002938$.

If m be negative, then $\frac{1}{x^{-n}\cdot\overline{ax^{r}-m}} = \overline{\frac{1}{ax^{n}-m} - \frac{1}{ax^{n}} - \frac{m}{a^{2}x^{2n}} - \&c.} \times \frac{a^{r}}{m^{r}}$.

Ex. 1. To find the ſum of $\frac{1}{1\cdot 2^{2}\cdot 3} + \frac{1}{2\cdot 3^{2}\cdot 4} + \frac{1}{3\cdot 4\cdot 5} + \&c.$ Here the general term is $\frac{1}{\overline{x-1}\times x^{2}\times\overline{x+1}} = \frac{1}{x^{2}\times\overline{x^{2}-1}} = \frac{1}{x^{2}-1} - \frac{1}{x^{2}}$; now, by writing 2, 3, 4, &c. for x, the ſum of the ſeries whoſe general term is $\frac{1}{x^{2}-1}$ is $= \frac{3}{4}$, by Prop. 2.; hence the ſum required $= \frac{3}{4} - A$.

Ex. 2. Let the given ſeries be $\frac{1}{1\cdot 2^{2}\cdot 3} + \frac{1}{3\cdot 4^{2}\cdot 5} + \frac{1}{5\cdot 6^{2}\cdot 7} +$ &c. Here the general term is the ſame as before, writing 2, 4, 6, &c. for x; and, by Prop. 2. the ſum of the ſeries whoſe general term is $\frac{1}{x^{2}-1}$ is $= \frac{1}{2}$; hence the ſum $= \frac{1}{2} - A''$.

Ex. 3. In like manner the ſum of the ſeries $\frac{1}{1\cdot 2^{4}\cdot 3} + \frac{1}{2\cdot 3^{4}\cdot 4} + \frac{1}{3\cdot 4^{4}\cdot 5} + \&c. = ,221689395104 - B$.

Ex. 4. To find the ſum of $\frac{1}{3\cdot 4\cdot 5} + \frac{1}{8\cdot 9^{2}\cdot 10} + \frac{1}{15\cdot 16^{2}\cdot 17} + \&c.$ Here the general term is $\frac{1}{\overline{x^{2}-1}\times x^{4}\times\overline{x^{2}+1}} = \frac{1}{x^{4}\times\overline{x^{4}-1}} = \frac{1}{x^{4}-1} - \frac{1}{x^{4}}$; but the ſum of the ſeries whoſe general term is $\frac{1}{x^{4}-1}$ is $= ,086662976264$; hence the ſum required $= ,086662976264 - C$.

PROP. V.

To find the ſum of the infinite ſeries $\frac{1}{15}+\frac{1}{40}+\frac{1}{85}+\frac{1}{156}+\frac{1}{259}+$ &c.

In this ſeries the fourth differences of the denominators $=0$; therefore the general term muſt be repreſented by $\frac{1}{ax^3+bx^2+cx+d}$; write therefore 2, 3, 4, &c. for x, and we have $8a+4b+2c+d=15$, $27a+9b+3c+d=40$, $64a+16b+4c+d=85$, $125a+25b+5c+d=156$; hence $a=1$, $b=1$, $c=1$, $d=1$, and the general term is $\frac{1}{x^3+x^2+x+1}=\frac{1}{x^3}-\frac{1}{x^4}+\frac{1}{x^7}-\frac{1}{x^8}+$ &c.; hence the ſum $=$ B $-$ C $+$ F $-$ G $+$ K $-$ L $+$ &c. $=$,1242700165.

PROP. VI.

To find the ſum of $\frac{2}{5^2}+\frac{3}{10^2}+\frac{4}{17^2}+$ &c. *ad inf.*

The general term $=\frac{x}{\overline{x^2+1}^2}=\frac{1}{x^3}-\frac{2}{x^5}-\frac{3}{x^7}+$ &c.; hence, by writing 2, 3, 4, &c. for x, we have the ſum $=$ B $-$ 2D $+$ 3F $-$ &c $=$,147115771469.

In like manner $\frac{2}{3^2}+\frac{3}{8^2}+\frac{4}{15^2}+$ &c. $=$ B $+$ 2D $+$ 3F $+$ &c. $=$,312498999865.

PROP. VII.

To find the ſum of $\frac{1}{3^2 \cdot 5^2} + \frac{1}{8^2 \cdot 10^2} + \frac{1}{15^2 \cdot 17^2} +$ &c. *ad inf.*

The general term is $\frac{1}{\overline{x^2-1}^2 \times \overline{x^2+1}^2} = \frac{1}{x^8} + \frac{2}{x^{12}} + \frac{3}{x^{16}} +$ &c. ; hence, by writing 2, 3, 4, &c. for x, we have the ſum $=$ G + 2L + 3P + &c. = ,00944769068 4.

PROP. VIII.

To find the ſum of $\frac{1}{1^3 \cdot 2^3 \cdot 3^3} + \frac{1}{2^3 \cdot 3^3 \cdot 4^3} + \frac{1}{3^3 \cdot 4^3 \cdot 5^3} +$ &c. *ad inf.*

Here the general term is $\frac{1}{\overline{x-1}^3 \cdot x^3 \cdot \overline{x+1}^3} = \frac{1}{x^9} + \frac{3}{x^{11}} + \frac{6}{x^{13}} +$ &c. and hence the ſum = H + 3K + 6M + &c. = ,004707148337.

PROP. IX.

To find the ſum of the infinite ſeries $1 - \frac{1}{3} + \frac{1}{6} - \frac{1}{10} +$ &c. *being a ſeries of the reciprocal of the figurative numbers of the* 3rd *order, having the ſigns alternately* + *and* −.

This ſeries, by reſolving two terms into one, becomes $\frac{4}{1 \cdot 2 \cdot 3} + \frac{4}{3 \cdot 4 \cdot 5} + \frac{4}{5 \cdot 6 \cdot 7} +$ &c. whoſe general term, by writing 2, 4, 6, &c. for x, is $\frac{4}{\overline{x-1} \times x \times \overline{x+1}} = \frac{4}{x^3} + \frac{4}{x^5} + \frac{4}{x^7} +$ &c. conſequently the ſum $= 4B'' + 4D'' + 4F'' +$ &c. = (by Cor. Ex. 2. Prop. 2.) $-2 + 4$ hyp. log. 2.

Cor. Hence, as $1 + \frac{1}{3} + \frac{1}{6} + \frac{1}{10} + \&c. = 2$, we have $1 + \frac{1}{6} + \frac{1}{15} + \&c. = 2$ hyp. log. 2, and $\frac{1}{3} + \frac{1}{10} + \frac{1}{21} + \&c. = 2 - 2$ hyp. log. 2.

PROP. X.

To find the ſum of the infinite ſeries $1 - \frac{1}{4} + \frac{1}{10} - \frac{1}{20} + \&c.$ *being the reciprocals of the figurative numbers of the 4th order, having the ſigns alternately + and −.*

If we write 2, −3, 4, −5, &c. for x, the general term will be $\frac{6}{x^3 - x} = \frac{6}{x^3} + \frac{6}{x^5} + \frac{6}{x^7} + \&c.$; hence the ſum required $= 6b + 6d + 6f + \&c. =$ (by Prop. 3.) $- 7\frac{1}{2} + 12$ hyp. log. 2.

Cor. Becauſe the ſum of $1 + \frac{1}{4} + \frac{1}{10} + \frac{1}{20} + \&c. = \frac{3}{2}$; therefore $1 + \frac{1}{10} + \frac{1}{35} + \&c. = -3 + 6$ hyp. log. 2; and $\frac{1}{4} + \frac{1}{10} + \frac{1}{56} + \&c. = 4\frac{1}{2} - 6$ hyp. log. 2.

PROP. XI.

To find the ſum of $\frac{2^2}{1^2 . 3^2} + \frac{3^2}{2^2 . 4^2} + \frac{4^2}{3^2 . 5^2} + \&c.$ *ad infinitum.*

The general term, by writing 2, 3, 4, &c. for x, is $\frac{x^2}{\overline{x-1}^2 \times \overline{x+1}^2} = \frac{1}{x^2} + \frac{2}{x^4} + \frac{3}{x^6} + \&c.$; hence the ſum $= A + 2C + 3E + \&c. = ,8849669934207$.

PROP. XII.

To find the ſum of $\frac{1}{1\cdot2^2\cdot3^3}+\frac{1}{2\cdot3^2\cdot4^3}+\frac{1}{3\cdot4^2\cdot5^3}+$ &c. *ad infinitum.*

Here the general term, by writing 2, 3, 4, &c. for x, is $\frac{1}{\overline{x-1}\cdot x^2\cdot\overline{x+1}^3}=\frac{1}{x^6}-\frac{2}{x^7}+\frac{4}{x^8}+\frac{6}{x^9}-\frac{9}{x^{10}}-\frac{12}{x^{11}}+\frac{16}{x^{12}}-$ &c.; conſequently the ſum $=E-2F+4G-6H+9I-12K+$ &c. $=$,01037089482.

PROP. XIII.

To find the ſum of $\frac{1}{2}A-\frac{1}{4}B+\frac{1}{8}C-$&c. *ad infinitum.*

The hyp. log. $2=1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\frac{1}{5}-$ &c. $=1+\frac{1}{3}+\frac{1}{5}+$ &c. $-\frac{1}{2}-\frac{1}{4}-\frac{1}{6}-$ &c.; hence $2\times$ hyp. log. 2, or hyp. log. 4, $=\frac{2}{1}+\frac{2}{3}+\frac{2}{5}+$ &c. $-1-\frac{1}{2}-\frac{1}{3}-$ &c. Now, by diviſion, [illegible] $=\frac{1}{x}-\frac{1}{2x^2}+\frac{1}{4x^3}-\frac{1}{8x^4}+$ &c.; hence, by writing 2, 3, 4, &c. for x, we have (after tranſpoſition) $\frac{2}{5}+\frac{2}{7}+$ &c. $-\frac{1}{2}-\frac{1}{3}-\frac{1}{4}-$ &c. $=-\frac{1}{2}A+\frac{1}{4}B-\frac{1}{8}C+$ &c.; hence, by adding equal quantities to each ſide, we have $\frac{2}{1}+\frac{2}{3}+\frac{2}{5}+$ &c. $-\frac{1}{2}-\frac{1}{3}-\frac{1}{4}-$ &c. $=\frac{8}{3}-\frac{1}{2}A+\frac{1}{4}B-\frac{1}{8}C+$ &c., conſequently $\frac{1}{2}A-\frac{1}{4}B+\frac{1}{8}C-$ &c. $=\frac{8}{3}-\frac{2}{1}-\frac{2}{3}-\frac{2}{5}-$ &c. $+1+\frac{1}{2}+\frac{1}{3}+$ &c. $=\frac{8}{3}-$ hyp. log. 4.

PROP. XIV.

To find the ſum of the infinite ſeries $\frac{1}{2\cdot5}+\frac{1}{3\cdot7}+\frac{1}{4\cdot9}+\&c.$

The general term, by writing 2, 3, 4, &c. for x, is $\frac{1}{x\cdot\overline{2x+1}}=\frac{1}{2x^2}-\frac{1}{4x^3}+\frac{1}{8x^4}-\&c.$; hence the ſum $=\frac{1}{2}A-\frac{1}{4}B+\frac{1}{8}C-\&c.=$ (by Prop. 13.) $\frac{8}{3}-$ hyp. log. 4.

PROP. XV.

To find the ſum of $1+\frac{1}{2}+\frac{1}{3}+\ldots\ldots$ *to* $\frac{1}{x}$.

The hyp. log. $\frac{x}{x-1}=\frac{1}{x}+\frac{1}{2x^2}+\frac{1}{3x^3}+\frac{1}{4x^4}+\&c.$; conſequently hyp. log. $\frac{x}{x-1}-\frac{1}{2x^2}-\frac{1}{3x^3}-\frac{1}{4x^4}-\&c.=\frac{1}{x}$; hence, if we write 2, 3, 4, &c. for x, we have hyp. log. $\frac{2}{1}$ + hyp. log. $\frac{3}{2}$ + &c. ....

$$\left.\begin{array}{l}\text{hyp. log. }\frac{x}{x-1}-\overline{\frac{1}{2}\times\frac{1}{2^2}+\frac{1}{3^2}+\&c.\ldots\ldots\frac{1}{x^2}}\\ \qquad\qquad -\overline{\frac{1}{3}\times\frac{1}{2^3}+\frac{1}{3^3}+\&c.\ldots\ldots\frac{1}{x^3}}\\ \qquad\qquad -\frac{1}{4}\times\frac{1}{2^4}+\frac{1}{3^4}+\&c.\ldots\ldots\frac{1}{x^4}\\ \qquad\qquad -\quad\&c.\quad\&c.\qquad\&c.\end{array}\right\}=$$

$\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\&c.\ldots\frac{1}{x}$; but hyp. log. $\frac{2}{1}$ + hyp. log. $\frac{3}{2}$ + hyp. log. $\frac{4}{3}$ + &c. .... hyp. log. $\frac{x}{x-1}$ = hyp. log. $\frac{2}{1}\times\frac{3}{2}\times\frac{4}{3}\times$ &c. .... $\frac{x}{x-1}$ = hyp. log. x; alſo $\frac{1}{2^2}+\frac{1}{3^2}+\&c.\ldots\frac{1}{x^2}=$ the ſum of the ſame ſeries *ad infinitum*, *minus* the ſum of all the terms from $\frac{1}{x} = (\text{if } x + 1 = n)\ A - \frac{1}{n} - \frac{1}{2n} - \frac{1}{6n^3} + \frac{1}{30n^5} - \frac{1}{42n^7} + \&c.$; in the ſame manner $\frac{1}{2^3} + \frac{1}{3^3} + \&c. \ldots \frac{1}{x^3} = B - \frac{1}{2n^2} - \frac{1}{2n^3} - \frac{1}{4n^4} + \frac{1}{12n^6} - \frac{1}{12n^8} + \&c.$; and ſo on for the other ſerieſes; hence, by ſubſtitution, and adding unity to each ſide, we have hyp. log. $x + 1 - \frac{1}{2}A - \frac{1}{3}B - \frac{1}{4}C - \&c. + \frac{1}{2n} + \frac{5}{12n^2} + \frac{1}{3n^3} + \frac{31}{120n^4} + \frac{1}{5n^5} + \frac{41}{252n^6} + \frac{1}{7n^7} + \frac{31}{240n^8} + \&c. = 1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c. \ldots \frac{1}{x}$; but $1 - \frac{1}{2}A - \frac{1}{3}B - \frac{1}{4}C - \&c. = ,577215664901$; hence $1 + \frac{1}{2} + \frac{1}{3} + \&c. \ldots \frac{1}{x} = \text{hyp. log. } x + ,577215664901 + \frac{1}{2n} + \frac{5}{12n^2} + \frac{1}{3n^3} + \frac{31}{120n^4} + \frac{1}{5n^5} + \frac{41}{252n^6} + \frac{1}{7n^7} + \frac{31}{240n^8} + \&c.$

Ex. 1. Let $x = 10000$; then

hyp. log. 10000 =	9,210340371976
conſt. quant. =	,577215664901
$\frac{1}{2n}$ =	,000049995000
$\frac{5}{12n^2}$ =	,000000004166
therefore the ſum required =	9,787606036043

Ex. 2. Let $x = 10000000$; then

hyp. log. 10000000 =	16,118095650958
conſt. quant. =	,577215664901
$\frac{1}{2n}$ =	,000000049999
therefore the ſum required =	16,695311365858

PROP.

PROP. XVI.

To find the value of $\alpha \times \beta \times \gamma \times \delta \times$ &c. *ad infinitum, ſuppoſing the general term to be a rational function of* x.

Let π be the general term, then reſolve $\frac{\dot{\pi}}{\pi}$ into an infinite ſeries, and take the fluent on both ſides; then write 2, 3, 4, &c. for x, and one ſide will become the hyp. log. of the given ſeries, and the value of the other ſide may be found from the tables.

Ex. 1. To find the value of $\frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times$ &c. *ad infinitum.*

Here the general term is $\frac{x^2}{x^2-1}$; hence $\frac{\dot{\pi}}{\pi} = -\frac{2\dot{x}}{x^3-x} = -\frac{2\dot{x}}{x^3} - \frac{2\dot{x}}{x^5} - \frac{2\dot{x}}{x^7} -$ &c.; hence the hyp. log. $\frac{x^2}{x^2-1} = \frac{1}{x^2} + \frac{1}{2x^4} + \frac{1}{3x^6} +$ &c. Write 2, 3, 4, &c. for x, and we have the hyp. log. $\frac{4}{3}$ + hyp. log. $\frac{9}{8}$ + hyp. log. $\frac{16}{15}$ + &c. $= A + \frac{1}{2}C + \frac{1}{3}E +$ &c. $= ,693147180574$, which is the hyp. log. 2; but hyp. log. $\frac{4}{3}$ + hyp. log. $\frac{9}{8}$ + hyp. log. $\frac{16}{15}$ + &c. = hyp. log. $\frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times$ &c. conſequently $\frac{4}{3} \times \frac{9}{8} \times \frac{16}{15} \times$ &c. $= 2$.

Ex. 2. To find the value of $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times$ &c. *ad infinitum.*

Here the general term is $\frac{x^3}{x^3-1}$; hence $\frac{\dot{\pi}}{\pi} = -\frac{3x}{x^4-x} = -\frac{3\dot{x}}{x^4} - \frac{3\dot{x}}{x^7} - \frac{3\dot{x}}{x^{10}} -$ &c.; hence the hyp. log. $\frac{x^3}{x^3-1} = \frac{1}{x^3} + \frac{1}{2x^6} + \frac{1}{3x^9} +$ &c. Write 2, 3, 4, &c. for x, and we have hyp. log. $\frac{8}{7}$ + hyp. log.

$\frac{27}{26}$ + hyp. log. $\frac{64}{63}$ + &c. = B + $\frac{1}{2}$E + $\frac{1}{3}$H + &c. = ,211466250444; or hyp. log. $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times$ &c. = ,211466250444; hence $\frac{8}{7} \times \frac{27}{26} \times \frac{64}{63} \times$ &c. = 1,627295, &c.

Hence we may find the value of ſuch a quantity, ſuppoſing the number of factors to be finite.

Ex. To find the value of $\frac{2}{1} \times \frac{4}{3} \times \frac{6}{5} \times$ &c. . . . . to $\frac{2x}{2x-1}$.

Here the general term being $\frac{2x}{2x-1}$, we have $\frac{\dot{\pi}}{\pi} = -\frac{\dot{x}}{2x^2 - x} = -\frac{\dot{x}}{2x^2} - \frac{\dot{x}}{4x^3} - \frac{\dot{x}}{8x^4} - \frac{\dot{x}}{16x^5} -$ &c.; hence hyp. log. $\frac{2x}{2x-1} = \frac{1}{1 \cdot 2x} + \frac{1}{2 \cdot 4x^2} + \frac{1}{3 \cdot 8x^3} + \frac{1}{4 \cdot 16x^4} +$ &c.. Now write 2, 3, 4, &c. for x, and we have the hyp. log. $\frac{4}{3} \times \frac{6}{5} \times \frac{8}{7} \times$ &c. . . . .

$$\left.\begin{array}{l} \frac{2x}{2x-1} = \frac{1}{2} \times \overline{\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \ldots\ldots \frac{1}{x}} \\ \quad + \frac{1}{2 \cdot 4} \times \overline{\frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \ldots\ldots \frac{1}{x^2}} \\ \quad + \frac{1}{3 \cdot 8} \times \overline{\frac{1}{2^3} + \frac{1}{3^3} + \frac{1}{4^3} + \ldots\ldots \frac{1}{x^3}} \\ \quad + \quad \text{\&c.} \qquad \text{\&c.} \qquad \text{\&c.} \end{array}\right\}$$

But, by Prop. 15. $\frac{1}{2} \times \overline{\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \ldots\ldots \frac{1}{x}} = \frac{1}{2}$hyp. log. $x -{,}211392167549 + \frac{1}{4n} + \frac{5}{24n^2} + \frac{1}{6n^3} + \frac{31}{240n^4} + \frac{1}{10n^5} + \frac{41}{504n^6} +$ &c.; alſo $\frac{1}{2 \cdot 4} \times \overline{\frac{1}{2^2} + \frac{1}{3^2} + \ldots\ldots \frac{1}{x^2}} = \frac{1}{2 \cdot 4} \times \overline{A - \frac{1}{n} - \frac{1}{2n^2} - \frac{1}{6n^3} + \frac{1}{30n^5} - \text{\&c.}}$ and $\frac{1}{3 \cdot 8} \times \overline{\frac{1}{2^3} + \frac{1}{3^3} + \ldots\ldots \frac{1}{x^3}} = \frac{1}{3 \cdot 8} \times \overline{B - \frac{1}{2n^2} - \frac{1}{2n^3} - \frac{1}{4n^4} + \frac{1}{12n^6} - \text{\&c.}}$, and ſo on for the other ſerieſes: hence, by ſubſtitution,

tion, hyp. log. $\frac{4}{3}\times\frac{6}{5}\times\frac{8}{7}\times$ &c. .... $\frac{2x}{2x-1} = \frac{1}{2}$ hyp. log. $x -$ $,12078223764 + \frac{1}{8n} + \frac{1}{8n^2} + \frac{23}{192n^3} + \frac{7}{64n^4} + \frac{61}{640n^5}$ + &c.; conſequently $\frac{4}{3}\times\frac{6}{5}\times\frac{8}{7}\times$ &c. .... $\frac{2x}{2x-1}$ = the natural number correſponding to the right hand ſide of the equation; hence $\frac{2}{1}\times\frac{4}{3}\times\frac{6}{5}\times$ .... $\frac{2x}{2x-1}$ = twice that natural number.

Ex. Let $x = 10000$; then

$$\begin{array}{rr} \frac{1}{2}\text{ hyp. log. } x = & 4,605170185988 \\ \text{conſt. quant.} = & ,120782237640 \\ \hline & 4,484387948348 \\ \frac{1}{8n} = & ,000012498750 \\ \frac{1}{8n^2} = & ,000000001249 \\ \hline & 4,484400448347 \end{array}$$

the natural number correſponding to which hyp. log. is 88,6238, &c., conſequently $\frac{2}{1}\times\frac{4}{3}\times\frac{6}{5}\times$ ..... $\frac{20000}{19999} = 177,2476$.

If x be a very large number, it may be ſufficiently exact in moſt caſes to take twice the natural number correſponding to the hyp. log. of $\frac{1}{2}$ hyp. log. $x - ,120782237640$.

XXI. *Experiments and Obſervations to inveſtigate the Compoſition of* James's *Powder. By* George Pearſon, *M. D. F. R. S.; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 23, 1791.

THE medicine upon which many phyſicians principally depend in the cure of continued fevers is JAMES's *Powder*; but, although it has been very extenſively uſed above thirty years, the public have not, I believe, been informed of the particular nature of this ſubſtance. This *powder* was originally a patent medicine; but it is well known that it cannot be prepared by following the directions of the ſpecification in the Court of Chancery. Preſuming that I have made ſome experiments and obſervations which may explain the nature and manner of preparing this medicine, and, perhaps, may extend the hiſtory of antimony; I beg leave to have the honour of preſenting an account of them to the Royal Society.

Senſible properties of JAMES's *Powder.*

Some parcels of this preparation are white, but in general it has a yellowiſh caſt; and this ſhade is more evident in ſome ſpecimens than in others. It is ſaid, that this powder cannot, in general, be made at different times of preciſely the ſame ſhade of yellow or degree of whiteneſs. Sometimes with the aid of a lens a few very ſmall ſhining *ſpicula* are ſeen mixed with powder. When preſſed between the fingers it feels ſmooth.

ſmooth, with ſome rather rough particles, and it is gritty in the mouth. Moſt parcels at firſt are taſteleſs; but in about a minute there is a ſlight braſſy taſte. It is perfectly inodorous.

Specific gravity.

This powder feels much heavier than any of the common earths and ſtones in a pulverized ſtate. One of the phials, nearly full, in which it is ſold, reckoned to hold a quantity equal to twelve packets, or 480 grains, contained 470 grains troy weight of JAMES's powder. This phial, filled with diſtilled water to the ſame height that it had been by the powder, was found to contain nearly four drachm-meaſures, or about 240 grains, of this liquid *.

Effects of fire.

(*a*) The exterior part of the flame of a candle applied, by means of the blow-pipe, to about one, two, and three grains of JAMES's Powder on charcoal, and alſo in the ſpoon, only made it yellowiſh while hot, but, on cooling, this colour diſappeared. The interior and hotteſt part of the blue flame turned this powder yellow, and when continued ſo as to ignite it, a white inodorous fume or vapour aroſe, which ſoon ceaſed; and though the heat was continued, the powder neither appeared to diminiſh nor melt; but, on cooling, a ſlightly cohering white ſolid, about three-fourths of the original weight of the powder, was left. If the flame was ſuddenly withdrawn, as ſoon as the white fumes appeared, they aſcended with a kind of revolving motion.

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* After this Paper had been read, an experiment, in a different manner, was made to aſcertain the ſpecific gravity of this powder. The quantity which nearly filled a phial weighed 437 grains; and filling the ſame phial, to the ſame height, with diſtilled water, the temperature of which was 65°, the water weighed 250,2 grains. The reaſon of the variation in theſe reſults, in making uſe of different parcels of this medicine, will be obvious, from the following account of its preparation, and the great difficulty of determining, with accuracy, the ſpecific gravity of powders.

(*b*) Two grains of this powder, mixed with about three of pulverized tartar, being expoſed on charcoal to flame applied by the blow-pipe, the mixture turned black, boiled, and ſwelled; and by continuing to apply the flame, the coaly matter of the tartar diſappeared, a part of the mixture fuſed, and in that ſtate ſeveral ſmall, ſilvery, apparently metallic grains were perceived. On cooling, they were ſeen with the naked eye, or with a lens, adhering to an irregularly figured, partially melted, whitiſh maſs. On a ſecond application of the flame, theſe metallic globules diſappeared.

(*c*) JAMES's Powder, with glaſs of phoſphoric acid, melted into an opaque yellowiſh globule while hot, which on cooling grew whitiſh.

(*d*) This Powder, with ſeveral times its weight of melted borax, afforded a colourleſs tranſparent glaſs while fluid; but, on adding a larger proportion of powder, the globule turned opaque, and when cold became of a milky whiteneſs. As the JAMES's Powder mixed, or melted, with the fuſed ſalt, ſlight exploſions were ſeveral times heard.

(*e*) With foſſil alkali, in the ſpoon, this powder apparently fuſed, and afforded a colourleſs tranſparent fluid in a ſtate of rotatory motion; but on cooling it grew opaque, and had a horny appearance.

(*f*) 100 grains of this powder in a two-inch Engliſh crucible, the cover of which was luted on it as cloſely as poſſible, were expoſed above two hours to a fierce fire in a melting furnace. On breaking this crucible, when cold, the powder was found changed into an entire very hard white ſolid, receiving its figure from the veſſel, and weighed 95 grains. On breaking this hard ſolid, the lower part of it ſeemed to be vitrified, or in a ſtate of enamel; and being powdered, it afforded a much whiter powder, and of greater ſpecific gravity, than before. The degree of fire denoted by WEDGWOOD's pyrometer was 166°.

Theſe experiments indicated the preſence of a metallic calx, a part at leaſt of which was that of antimony, mixed with earthy matter.

Experiments with different menſtrua applied to JAMES's *Powder.*

I. *With water.*

300 grains of this powder were digeſted for ſeveral hours in twenty-ſeven ounces of diſtilled water, and then boiled for one hour. While boiling the water appeared milky; but in half a minute's time, after withdrawing the lamp over which it boiled, the liquid became nearly clear, and the ſediment depoſited was apparently the powder undiminiſhed in quantity, and in other reſpects unaltered. While hot the liquid was decanted upon a filter of ſeveral folds of paper previouſly weighed, through which twenty ounces of quite clear liquid, like water, readily paſſed. Very little powder could be perceived on the filter; but when it was well dried, it weighed fourteen grains more than before the experiment. The filtered liquor was taſteleſs. In about three quarters of an hour it grew ſlightly *turbid*, and in ten minutes after became *milky*. On ſtanding eight days longer, the milkineſs diminiſhed, and a ſmall quantity, perhaps four grains, of cloſe white ſediment, firmly adhering to the ſides of the veſſel, were depoſited. A little of this ſlightly milky fluid being made hot, it grew quite clear, and on cooling

turned milky as before, or depoſited a ſediment; therefore, this milkineſs depended on cold water diſſolving a ſmaller proportion of JAMES's Powder than hot water. The whole of the filtered milky liquor was poured upon a filter as before, through which it paſſed without any diminution of its turbid appearance; and, on drying, the filter was found to have increaſed only a quarter of a grain in weight. Some of the vapour that aroſe during the ebullition being condenſed was found to be pure water.

On a repetition of this experiment the phænomena above related were always obſerved; but the ſemi-vitrified JAMES's Powder above-mentioned (*f*) afforded a much leſs milky fluid and ſediment than the powder uſed in the preceding experiment.

In order to determine the kind of ſubſtances in water after boiling in it JAMES's Powder, the following re-agents were added to the above filtered liquor.

1. Acid of ſugar ſometimes occaſioned immediately more turbidneſs, and at other times tranſparency was inſtantly produced; but in all caſes, on ſtanding, more ſediment fell than from the filtered liquor alone.

2. Muriated barytes in about an hour rendered this liquid evidently more turbid, and on ſtanding more ſediment was depoſited than from the filtered liquid to which nothing had been added.

3. Lime-water occaſioned immediately a curdy appearance.

4. Infuſion of turnſole was ſometimes turned to a ſlight red; but in general it was not altered in colour.

5. Nitre of ſilver produced in a few hours a ſlight ſediment.

6. Pruſſianated alkali of tartar occaſioned no alteration immediately, nor for four days after adding acetous acid to this mixture.

7. Mild alkali of tartar, and likewiſe mild foſſil alkali, though boiled in this liquor, did not occaſion any additional precipitation.

The ſediment that fell, on merely ſtanding, from the above filtered liquid, was next examined.

1. It did not diſſolve in 100 times its weight of boiling hot concentrated acetous acid; nor,

2. Was it apparently acted upon by boiling with mild alkali of tartar; for, after decanting the clear liquid of this mixture, the ſediment from it was not diſſolved by a large quantity of acetous acid, but readily by marine acid.

3. This ſediment immediately diſappeared on pouring upon it a much ſmaller quantity of marine acid, and alſo of acid of nitre, than had been applied of acetous acid, without any ſolution enſuing.

4. To one portion of this ſolution in marine acid juſt mentioned, was added gradually lixivium of alkali of tartar; after the effervefcence had ceaſed to be produced, the firſt drop occaſioned a turbid appearance, and the liquid did not grow clear again on adding a large proportion of vinegar to make it ſour.

5. Another portion of this ſolution in marine acid, being boiled to carry off the ſuperabundant acid, was poured into a large proportion of lime-water, by which it was rendered cloudy, and did not become clear again on adding concentrated acetous acid to make it ſour.

6. To a third portion of this ſolution in marine acid, from which the redundant acid had been carried off, twenty drops of Pruſſian alkaline lixivium were added, which immediately turned it of a bluiſh caſt without diſturbing its tranſparency; and, after ſtanding four days, a ſmaller quantity of pale blue

ſediment was depoſited than had fallen from a quantity of diſtilled water equal to this muriatic ſolution, to which had been added twenty drops of this Pruſſian alkaline lixivium, and one-fourth part of a drop of muriated antimony. The ſame Pruſſian alkali mixed with vinegar, on ſtanding, turned bluiſh, but depoſited nothing.

7. Though the ſediment that fell in the filtered fluid, p. 320. on merely ſtanding, diſſolved in the nitrous acid as above mentioned; yet, when this acid was added in a ſmaller proportion, but to render the mixture ſour, a partial ſolution only took place. On adding, however, a very ſmall quantity of marine acid, the ſolution was total, and with leſs ſuperabundance of this menſtruum than of the nitrous acid in which a part remained undiſſolved. This ſolution

(*a*) With water depoſited Algaroth powder:

(*b*) With Pruſſian alkali turned bluiſh, and, on ſtanding, a ſmall quantity of ſediment took place.

(*c*) A bright plate of copper, immerſed in this ſolution, did not appear to be at all whitened, or rendered paler.

(*d*) Muriated barytes rendered this ſolution very turbid inſtantly.

(*e*) Acid of ſugar produced no change, except in two or three days a very minute portion of ſediment; but the ſaccharated ſoda immediately occaſioned a ſlight precipitation.

(*f*) Alkali of tartar, fully aërated, rendered this ſolution ſlightly turbid; but cauſtic mineral alkali induced no change.

(*g*) Lime-water, in a ſmall proportion, did not affect the tranſparency; but in a larger produced copious clouds.

The JAMES's Powder which afforded the ſolution in water, on which the experiments above related were made, was boiled a ſecond time in eighteen ounces of diſtilled water for two hours. The decanted and filtered liquid on cooling grew leſs turbid than before, p. 320. and depoſited leſs ſediment. The filter, on drying, was found to have gained ten grains, though very little powder could be ſeen upon it. The experiments above related, were made on this filtered liquor, and in a ſlighter degree the ſame appearances were obſerved.

The JAMES's Powder remaining after theſe experiments, being well dried, weighed 260 grains, and therefore was found to have loſt 40 grains, partly by ſolution in water, but ſtill more by its adhering to the filters.

I wiſhed to know the proportion in which JAMES's Powder diſſolved in water; and therefore evaporated, in a very thin light glaſs pan, previouſly weighed, twenty-four ounces of the filtered liquids, p. 320. and p. 324. Very little precipitated matter appeared till nearly the whole of the liquor was evaporated; and, when the whole was carried off, a taſteleſs whitiſh, leafy, or mica-like ſediment, but in ſome parts black, was left, that weighed ſix grains. This ſediment required above 100 times its weight of hot water to diſſolve it. It was boiled in 500 times its weight of diſtilled water, and it paſſed through ſeveral folds of filtering paper rather turbid even while hot; nor could it be rendered clear by repeated filtration through paper. This filtered liquid,

(*a*) With infuſion of turnſole and turmeric, betrayed no alkaline ſubſtance, nor deciſively any acidity.

(*b*) Lime-water rendered it curdy; and on adding vinegar, it grew milky.

(*c*) With acid of ſugar it grew clear; but, on ſtanding, was more turbid than before.

(*d*) Salited barytes made it inſtantly turbid.

(*e*) Alkalies, mild and cauſtic, induced no change.

(*f*) Pruſſian alkali produced only a clear greeniſh colour, after the addition of vinegar, and long ſtanding.

(*g*) A very ſmall quantity of marine acid rendered it quite clear; but it required much more nitrous acid to produce this effect; and this mixture did not whiten copper.

(*h*) With nitre of ſilver the filtered liquor turned of a ſomewhat bluiſh hue, and afterwards curdy.

The undiſſolved matter that remained on the filter, p. 324. l. 22. above mentioned, appeared, on examination, to be the ſame kind of ſubſtance, with a larger proportion of iron, as that which was diſſolved by water, the experiments on which have been juſt mentioned. In particular, it afforded Algaroth powder, but did not whiten, in the ſmalleſt degree, a copper plate.

The following concluſions may, perhaps, be juſtifiably drawn from theſe experiments on JAMES's Powder with water.

1. That the whole, or a part, is ſoluble, or at leaſt may be ſuſpended, in about 2000 times its weight of pure water cold; and in about half this quantity of boiling water.

2. That this ſolution contains calcareous earth united to an acid, or ſome other ſubſtance, from which it cannot be diſunited by cauſtic or mild fixed alkalies; therefore, the precipitation by muriated barytes cannot be referred to vitriolated lime.

3. That this ſolution contains a metallic calx, a part of which at leaſt is that of antimony uncombined, or at leaſt not united to any acid with which it forms a compound ſoluble in water.

4. That the ſubſtance in the nitrous ſolution of the part of JAMES's Powder that had been diſſolved in water, which precipitates lime from lime-water, and which precipitate is not ſoluble in a large quantity of vinegar, is, probably, phoſphoric acid from phoſphorated lime decompoſed by nitrous acid.

The precipitation by muriated barytes and nitrated ſilver could not be from vitriolic and marine acids conſiſtently with the preceding experiments; and I could not have conjectured what was the ingredient in JAMES's Powder which occaſions it, if I had not found, that muriated barytes is not only a teſt of vitriolic but of phoſphoric acid united to lime and alkalies; and the acid of phoſphorus will alſo produce a turbid appearance with nitrated ſilver. The calx of iron, in the above experiments, is in perhaps too ſmall a quantity to be conſidered in any other light than as an accidental ſubſtance.

II. *With acetous acid.*

The 260 grains of JAMES's Powder, remaining after boiling 300 grains of it in diſtilled water, and after the unavoidable waſte of it in the above experiments with water, were put into a tubulated retort that would contain four ounce-meaſures, on which were poured three ounces of concentrated acetous acid, the ſpecific gravity of which was as 106 to 100, the neck of the retort being immerſed in quickſilver, and the tubulated part being immediately cloſely ſtopped. No efferveſcence was perceived; nor did any elaſtic fluid riſe during twenty-four hours into an inverted veſſel of quickſilver; and when heat was applied to boil the acid, nothing but the common air of the retort and a little water and acetous acid came over.

This mixture of acetous acid and powder being poured, while hot, on a filter of two folds of paper, a clear and colourleſs liquid paſſed through, that remained ſo, when cold, without any ſediment. The filter, with the powder upon it, being well dried, weighed ten grains more than the ſum of their weight ſeparately before the experiment; but the powder being carefully ſcraped off weighed only a little more than 240 grains, and appeared to have ſuffered no change in its properties.

This filtered liquor was ſubjected to diſtillation; it remained clear till about half of it had come over: but then it became rather turbid, and grew more ſo to the end of the diſtillation. There remained in the retort apparently four or five grains of brown ſediment, that adhered very cloſely to the bottom and ſides of it, nearly half as high as the liquid reached.

1ſt, This acetous acid, in which James's Powder had been boiled, and afterwards diſtilled, was found to contain no earth, ſalt, or metallic matter: nor did the acid itſelf appear to be altered in its chemical qualities.

2dly, The reſiduum in the retort had no taſte. It did not appear to diſſolve by trituration in one ounce of diſtilled water, nor in lixivium of alkali of tartar, mild or cauſtic. After boiling this reſiduum in one and a half ounce of water, part of it ſeemed to be diſſolved; and this ſolution being filtered was examined with the following re-agents.

(*a*) Acid of ſugar rendered it turbid, and tranſparency did not enſue on adding a further quantity of this acid.

(*b*) Muriated barytes produced a conſiderable turbidneſs, which was not removed by adding concentrated acetous acid.

(*c*) Nitrated ſilver induced a ſlight turbid appearance.

(*d*) Mild alkalies induced no viſible change.

(*e*) Pruſſianated foſſil alkali occaſioned a very ſlight opal coloured appearance, and after ſtanding ſix days a very ſmall quantity of whitiſh ſediment was thrown down, which diſſolved on concentrated acetous acid being added, and the liquid turned greeniſh; but, after ſtanding, a little greeniſh ſediment was depoſited, not, however, apparently more than was produced by a mixture of this Pruſſian alkali, acetous acid, and water.

(*f*) Phoſphorated foſſil alkali produced no turbidneſs or precipitation on ſtanding a week; but on the addition of $\frac{1}{100}$ gr. of nitrated mercury (which had been made by fully ſaturating the nitrous acid with mercury) the mixture grew inſtantly thick, and depoſited a copious white ſediment.

3dly, The remainder of the reſiduum, in the retort above mentioned to have been left after diſtilling to dryneſs acetous acid in which JAMES's Powder had been boiled, did not totally diſſolve in a large proportion of nitrous acid diluted; but was wholly taken up on adding a little marine acid. A great part of the ſuperabundant acid of this ſolution being carried off by evaporation, it was examined with the following ſubſtances.

(*a*) Adding a little of it to a large proportion of water, milkineſs enſued.

(*b*) The ſame appearance took place with a large proportion of lime-water.

(*c*) A turbid bluiſh colour was produced on adding Pruſſian mineral alkali, and on ſtanding a bluiſh ſediment took place.

(*d*) A poliſhed copper plate was not at all whitened by immerſion in this ſolution; but, on adding to it $\frac{1}{400}$ of a grain of nitre of mercury, the copper was rendered paler coloured.

The deficiency of twenty grains of powder in theſe experiments with acetous acid muſt be aſcribed partly to the ſolution in this menſtruum, and ſtill more to the waſte in the paper filter, and to its adhering to the retort when firſt boiled.

Theſe experiments indicated the ſame kind of ſubſtances as the experiments with water, namely, calcareous earth in a combined ſtate; phoſphoric acid; calx of antimony and of iron. It appears alſo, that JAMES's Powder is either wholly or partially ſoluble in about 300 times its weight of concentrated acetous acid.

III. *With nitrous acid.*

The 240 grains of powder remaining after the experiments with acetous acid, p. 327. were digeſted in the cold twelve hours, in three ounce meaſures and a half of purified and concentrated nitrous acid, diluted with four ounces of pure water, and then diſtilled with a gentle heat till there remained about two ounce meaſures. After ſtanding twenty-four hours, about one ounce and a half of clear liquid, which was very corroſive and acid, was drawn off by means of a ſyphon. The turbid liquid and cloſe white ſediment remaining, being mixed with one ounce of diſtilled water, were poured upon a filter of paper, and hot diſtilled water was repeatedly poured upon this filter till it paſſed through almoſt taſteleſs. The firſt portions of the filtered liquid, in quantity ten ounces, being mixed together, were ſet to evaporate in a glaſs pan. As ſoon as the liquor grew hot, the turbidneſs diſappeared; and as the evaporation went on, firſt clouds, and afterwards portions of ſediment, appeared in a clear liquid. The evaporation being continued nearly to dryneſs, a white, porous, or cellular cake was left, that weighed 129 grains. The liquid obtained by diſtillation from the ſolution which left this maſs was found to be merely diluted nitrous acid.

The reſiduum left on the filter which had reſiſted ſolution in nitrous acid, being well dried, weighed a little more than 142 grains. This reſiduum was digeſted, and boiled as before in nitrous acid; and this menſtruum, diſtilled from the reſiduum, being evaporated to dryneſs, afforded 6,5 grains of a whitiſh maſs. The reſiduum left on the filter after this ſecond application of nitrous acid, being well dried, weighed 132 grains.

The maſs of 129 grains, left on evaporation of the above ſolution of JAMES's Powder in nitrous acid, in a few hours began to deliqueſce, eſpecially at the edges. Some of the deliqueſcing part of the maſs was diſſolved in one ounce and a half of water, forming an opal-coloured ſolution, with a white ſediment. This opal-coloured ſolution being filtered was examined.

(*a*) It rendered lime-water milky; and the milkineſs did not diſappear on adding concentrated acetous acid, but readily on pouring into the mixture a little acid of nitre.

(*b*) It turned thick and white, and ſoon depoſited a copious ſediment of white matter, with a few drops of nitrous ſolution of mercury; and became turbid alſo with nitrated ſilver.

(*c*) With muriated barytes it became very turbid, and remained ſo after adding acetous acid; but grew clear again on adding nitrous acid.

(*d*) Acid of ſugar produced a turbid appearance.

(*e*) Cauſtic volatile alkali produced very little precipitation; but a copious one took place with mild alkali of tartar; which precipitated matter, after decanting the clear ſolution, was nearly all diſſolved by acetous acid, and the remainder was readily taken up by the marine as well as by the nitrous acid.

(*f*) Pruſſian alkali occaſioned a light blue colour, but no turbid appearance till the mixture had ſtood ſeveral days, which was then ſlight.

(*g*) No change of colour was produced on a copper plate.

A little of the ſoft and deliqueſcing part, juſt mentioned to have taken place at the edges of the ſolid maſs, taſted bitter and ſour. It melted under the blow-pipe into a horny kind of globule; but the dry part of this maſs could not be fuſed by this means.

This ounce and a half of ſolution being conſumed in theſe trials, the ſame quantity of boiling diſtilled water was poured on the precipitate or part not diſſolved by this quantity of water on the firſt affuſion. After ſtanding and boiling, the precipitate appeared to be but little diminiſhed. The clear liquid did not as before render lime-water turbid; but a precipitate enſued with nitrated mercury, which on compariſon was found to be a more delicate teſt of phoſphoric acid than lime-water. This ſolution alſo, by this ſecond affuſion of water, did not as before grow thick with mild alkali of tartar, nor blue with Pruſſian alkali. The ſediment, undiſſolved by theſe two applications of water, did not totally diſſolve in a ſuperabundant quantity of nitrous acid; but completely and immediately in a ſmaller quantity of marine acid; and this ſolution in marine acid, with a large proportion of water, produced milkineſs; with Pruſſian alkali, it turned of a deep blue colour; it did not whiten copper; ſaccharine acid and ſalited barytes only ſlightly diſturbed its tranſparency.

By theſe experiments I found the ſolution of JAMES's Powder in nitrous acid contained, probably, a pretty conſiderable proportion of calcareous earth united to both nitrous and phoſphoric acid; a little phoſphoric acid in a free ſtate; and a ſmall proportion of calx of antimony and of iron.

Ninety grains of the dry part of the above maſs of 129 grains, p 329. were repeatedly triturated and digeſted in alcohol till almoſt nothing was taken up by it. This ſolution, being filtered, was evaporated to dryneſs, and afforded 20 grains and a half of a fine white ſalt, very bitter, which, on expoſure to the air, ſoon became liquid, but very turbid.

The powder that had thus ceaſed to yield any thing further to alcohol was repeatedly triturated and boiled in pure water, till the liquid paſſed taſteleſs through the filter; and the filtered liquors, being evaporated, left eight grains more of a leſs bitter and leſs deliqueſcent ſubſtance than that from alcohol. Part only of theſe eight grains was ſoluble readily in water; and they appeared to be a mixture of the ſaline matter diſſolved in alcohol, and of the inſoluble reſiduum in that menſtruum.

The powder remaining on the filter after theſe ſolutions in alcohol and water being dried, weighed nearly 59 grains. It was white and taſteleſs.

I next examined theſe products more particularly; and *firſt* the twenty grains and a half which had been diſſolved in alcohol.

(*a*) With a large proportion of water it produced a rather turbid appearance, and, after ſtanding, a ſediment of calcareous earth was depoſited.

(*b*) This laſt ſolution (*a*) being filtered, with mild alkalies grew very thick, and depoſited a ſediment that was readily taken up by acetous acid.

(*c*) With cauſtic volatile alkali its tranſparency was ſcarcely diſturbed.

(*d*) With acid of ſugar it became thick and white; and

(e) White as cream of milk with phoſphorated mineral alkali; the ſediment from which mixture did not diſſolve in a quantity of boiling water that would have diſſolved vitriolated lime, nor of vinegar, but was readily taken up by nitrous acid.

(f) The tranſparency of nitrated and muriated barytes was ſcarcely diſturbed.

(g) It turned infuſion of turnſole red.

(h) A little of the deliqueſcent ſalt above mentioned, that had been diſſolved in alcohol, being made nearly dry, on adding to it a mixture of alcohol and acid of vitriol, vapours of nitrous ether were detached with ebullition

(i) With lime-water it produced a ſlight ſediment.

(k) With Pruſſian alkali at firſt a pale green colour, and afterwards a blue colour was produced; but without any precipitation on ſtanding.

(l) This ſubſtance, which had been diſſolved in alcohol, was infuſible under the blow-pipe; and after being heated red-hot on charcoal it was no longer ſoluble in water. Being further examined, it was found to be merely calcareous earth.

This ſoluble part then in alcohol appeared to be nothing but nitrated lime, with ſome traces of calcined iron.

Secondly, The 59 grains of powder, not ſoluble in alcohol, were examined.

(a) A mixture of vitriolic acid and alcohol detached from this powder no nitrous ether, nor any vapour that formed white clouds with volatile alkali.

(b) It did not efferveſce, and required above 200 times its weight of concentrated acetous acid to diſſolve it.

(c) Under the blow-pipe it emitted no ſmell or fume, and with great difficulty melted imperfectly, affording an irregular figured, horn-like, opaque maſs.

(*d*) It was not diminiſhed or altered in its properties by boiling in lixivium of alkali of tartar.

(*e*) Nitrous acid formed with it, without efferveſcence, a very ſlightly turbid ſolution; which ſolution produced the ſame appearances as thoſe related, p. 330, 331.

(*f*) 50 grains of this powder were diſſolved in nitrous acid, and a great part of the redundant acid being carried off by evaporation, to one half of this ſolution was added lime-water till it ceaſed to produce any milkineſs, and the mixture taſted of lime-water. After ſtanding excluded from the air, the ſediment depoſited from a clear liquid was collected, and being dried it weighed 26,3 grains. This precipitate had the properties of phoſphorated lime, with that proportion of lime and phoſphoric acid which forms a compound ſcarcely fuſible. The liquid in which this precipitate fell ſeemed to contain a little phoſphorated lime, but principally calcareous earth.

(*g*) To the other half of this ſolution in nitrous acid (*f*) was added vitriolic acid, drop by drop, till it no longer diſturbed its tranſparency. After ſtanding, the clear liquid was decanted from the precipitated matter which had taken place, and the precipitate with a ſmall quantity of water was thrown upon a filter. The filtered and decanted liquids mixed together were boiled till the ſmell of nitrous acid ceaſed, and there remained about half an ounce of acid liquor, which being filtered to ſeparate the vitriolated lime precipitated during evaporation, foſſil alkali was added to perfectly ſaturate it. During this union there was an efferveſcence and a ſeparation of more vitriolated lime, which being removed, the ſaturated liquor, by cryſtallization, afforded nearly 26 grains of cryſtals of phoſphorated foſſil alkali, beſides

ſides a little cubic nitre, vitriolated foſſil alkali and iron, with ſome veſtiges of calx of antimony, and phoſphorated lime.

The precipitate thrown down on adding vitriolic acid, and left upon the filter, weighed when dried 26,1 grains, and was vitriolated lime, with a minute portion of calcined antimony and iron.

The 6,5 grains, p. 330. left on evaporation to dryneſs of the ſecond ſolution in nitrous acid, conſiſted of nearly three grains of calcined antimony, and the reſt phoſphorated lime, with a little iron.

1. It appears from the above experiments with nitrous acid, that this menſtruum, by two affuſions, in a large proportion, aided by trituration, digeſtion, and heat, diſſolved $\frac{108}{240}$ of JAMES's Powder that had been expoſed to the action of water and acetous acid; but from the ſmallneſs of the quantity contained in the nitrous acid the ſecond time it was applied, and from its being principally calcined antimony, not more than two of the ſix grains afforded by this ſolution, perhaps, ſhould be conſidered to be *diſſolved*, for the reſt may be ſuppoſed to be merely *ſuſpended*.

The firſt ſolution alſo in this menſtruum was not filtered, and the acid was conſiderably redundant, and there was found in it ſeveral grains of calcined antimony. The real quantity *diſſolved* might therefore probably be eight grains leſs than the above 108 ſtated. According to this mode of calculation, the proportion of the ſoluble part of JAMES's Powder in nitrous acid is $\frac{100}{240}$, or about $\frac{41}{100}$.

2. The whole of this ſoluble part, except a little calx of antimony, is, deciſively, phoſphoric acid and calcareous earth:

which two ſubſtances may reaſonably be ſuppoſed, from theſe experiments, to have been united together, and to have been in the ſtate of phoſphorated lime in this powder. Conſequently, the proportion of this phoſphorated lime, conſidered as the ſoluble part of JAMES's powder in theſe experiments with nitrous acid, appears to be 40 *per cent.* making a deduction of 1 *per cent.* for the antimonial calx contained in the nitrous acid in the above experiments. It is however already obvious to ſuſpect, that the powder which reſiſted ſolution in this menſtruum may contain more phoſphorated lime; and this conſideration prevents me aſſigning at preſent the above 40 *per cent.* as the whole quantity of it in JAMES's Powder. It cannot however, I think, be a ſmaller proportion.

I do not reckon the calx of iron in theſe calculations, becauſe it is in too ſmall a quantity, and is apparently only to to be looked upon as an accidental extraneous ſubſtance. I ſuppoſe too, that the water and acetous acid applied to the JAMES's Powder uſed in theſe experiments, carried off a proportion of its ingredients equal to that in the remaining powder.

IV. *With marine acid.*

The 132 grains of heavy, white, taſteleſs powder, the reſiduum after boiling 240 grains of JAMES's Powder in nitrous acid, till it had diſſolved that part for which it has any conſiderable affinity, p. 330. were digeſted for twenty-four hours in eight ounce-meaſures of marine acid, the ſpecific gravity of which was 1,170, and diluted with half its bulk of diſtilled water. This mixture was diſtilled in a gentle heat till there remained about two ounce-meaſures of a very turbid liquor. After ſtanding in a jar two days, it depoſited a cloſe white ſediment, obviouſly much leſs in bulk than the powder added to this menſtruum; and nearly one ounce meaſure and three quarters of clear yellow liquid were drawn off, by means of a ſiphon, which was marked N° 1.

The diſtilled liquid, which was merely diluted marine acid, was poured back on the ſediment and remaining liquid; and after digeſtion twenty-four hours, this mixture was diſtilled as before, till there remained about one and a half ounce-meaſure; but after ſtanding in a jar ſeveral days, the quantity of ſediment depoſited was apparently as great as before the ſecond application of this menſtruum. The clear liquor was drawn off as before, and marked N° 2. The diſtilled liquor being found to be merely diluted marine acid, was poured on the remaining liquid and ſediment a third time; and, after digeſtion, the diſtillation was repeated as before. The remaining liquid having ſtood upon the ſediment ſome time, one ounce-meaſure of clear liquor was drawn off, and marked N° 3. The ſediment did not appear diminiſhed by this third diſtillation; but, as the decanted liquid, N° 3. was found to contain a ſmall quantity of ſome ſubſtance diſſolved or ſuſpended in it, the marine acid diſtilled in this experiment was poured a fourth time on this reſiduum, and after digeſtion boiled. Having ſtood ſeveral days, the clear liquor was decanted, and marked N° 4. To the reſiduum, after theſe four affuſions of marine acid, one ounce of boiling diſtilled water was added, and this mixture was poured on a filter. The powder upon the filter being well dried, was found to weigh 60,1 grains.

In the ounce of boiling water that had been filtered from this reſiduum, I could find nothing but ſome minute particles of that ſubſtance and veſtiges of iron. Two or three drops of the liquor, N° 1. added to three ounces of diſtilled water, produced a pretty conſiderable milkineſs; and, on ſtanding, a cloſe white ſediment was depoſited. Two or three drops of the liquor, N° 2. produced leſs milkineſs and ſediment in the quantity of water juſt mentioned than N° 1. N° 3. ſcarcely diminiſhed the tranſparency of diſtilled water; and N° 4. did not affect it all. The liquids, N° 3. and 4. were diſtilled till there remained about two drachm-meaſures of clear brown liquid, with a cloudy ſediment. N° 3. being poured into two ounces of diſtilled water ſcarcely made it milky; and N° 4. did not diminiſh the tranſparency of water at all. On evaporating to dryneſs theſe mixtures of the liquors, N° 3. and 4. with diſtilled water; that with N° 3. left a yellowiſh dry ſediment which weighed 3,1 grains; and that of N° 4. left 2,2 grains of ſediment. Nothing but calx of antimony could be diſcovered in theſe ſediments. They were reducible readily with tartar; ſcarcely fuſible in the ſpoon; but with phoſphoric acid eaſily melted into an opaque yellowiſh globule.

The liquor, marked N° 1. being poured into twenty-four ounces of diſtilled water produced a very milky appearance; and the ſame appearance, but in a much leſs degree, enſued on pouring the liquor N° 2. into this quantity of pure water. After ſtanding ſeveral days, a white ſediment being depoſited from a clear watery fluid, the clear liquid was drawn off by means of a ſiphon, and the ſediments being dried, that of N° 1. weighed 51 grains; that of N° 2. weighed 6,15 grains; and both were found to be purely Algaroth powder. The water, *viz.* 48 ounces, in which theſe precipitates fell, being evaporated to dryneſs, left a little more than four grains of an infuſible kind of ſediment, which was calx of antimony, like that of N° 3. and 4. with a minute portion of Algaroth powder and iron.

The 60,1 grains, p. 337. which reſiſted ſolution in marine acid, were a lighter powder than JAMES's Powder itſelf, rather gritty, quite taſteleſs. This powder was digeſted in one-ounce meaſure of concentrated nitrous acid mixed with two of acid of ſalt for a week; and then this mixture was diluted with four ounces of diſtilled water, and gently boiled till it was reduced to about two ounce-meaſures. On cooling and ſtanding ſeveral days, a ſediment of the ſame kind apparently, and in the ſame quantity as before evaporation, was depoſited, and, after ſtanding, a clear yellow liquid was drawn off. The ſediment, well dried, weighed 55 grains. The decanted liquor being reduced by evaporation from one and a half ounce to about two drachm-meaſures, a ſediment, while hot, appeared, which was calx of antimony, with a minute portion of earthy matter that had ſome properties of phoſphorated lime and calx of iron. A little of the clear liquor on this ſediment being dropped into water produced no milkineſs, nor ſediment, after ſtanding; but the whole of this clear liquor, with ſediment, being evaporated to dryneſs, afforded four grains of the ſame kind of infuſible calx of antimony, mentioned to have been obtained from the acid liquors, N° 3. and 4. above mentioned.

It appears then, that by repeatedly digeſting and boiling in marine acid, and in *aqua regia*, that part of JAMES's Powder which reſiſted ſolution in nitrous acid, which was $\frac{132}{240}$, p. 335. 77 grains were carried off by theſe menſtrua; but conſidering the ſmall proportion contained in theſe acids after the two firſt affuſions, which afforded 57,15 grains, p. 338., and ſuppoſing the calx to be neither increaſed nor diminiſhed in weight by the acids, the real quantity of ſoluble and fuſible calx of antimony may be ſtated to be that of Algaroth powder; for the other kind of antimonial calx obtained by ſubſequent affuſions was probably only *ſuſpended.* Conſequently 240 grains of JAMES's Powder afforded, by the above experiments with marine acid, 57,15 grains of Algaroth powder, and 19,85 grains of a leſs ſoluble and more difficultly fuſible calx of antimony, with a ſmall proportion of phoſphorated lime. The reſiduum, amounting to 55 grains, was of courſe next examined.

Experiments upon the part of JAMES's *Powder which was inſoluble in the above menſtrua.*

(*a*) A few grains of this inſoluble ſubſtance could neither be melted nor carried off in vapour by means of the flame of a candle applied to it upon charcoal, and alſo in the ſpoon, with the blow-pipe; but,

(*b*) Mixed with an equal weight of tartar, it melted on charcoal; and, while in fuſion, ſmall, apparently, metallic grains were diſtinctly perceived; and on cooling they could be ſeen, even without a lens, adhering to an irregularly figured, opaque, whitiſh maſs. Sometimes ſlight exploſions were heard while the flame was applied. The metallic grains appeared more diſtinctly when this powder was mixed with one-third of its weight of powdered calcined bone, than in JAMES's Powder.

(*c*) Ten grains of this powder were melted as above mentioned, by repeatedly applying flame with the blow-pipe to two or three grains of it at a time mixed with tartar. The opaque whitiſh maſſes, with metallic grains in them, thus obtained, being pulverized, were digeſted and gently boiled in diluted nitrous acid. The filtered ſolution afforded nitre, and nitrous acid in a free ſtate, the greateſt part of which ſuperabundant acid being carried off, the lixivium did not whiten copper, or throw down any calx but iron with Pruſſian alkali. The reſiduum that reſiſted ſolution in nitrous acid was digeſted and gently boiled in *aqua regia.* On ſtanding it was decanted, and this decanted liquid being heated, to carry off ſuperabundant acid and water, it afforded on mixture with water 1,2 grain of Algaroth powder, and no metallic matter could be detected in the water excepting a little iron. A ſmall part only being diſſolved by the *aqua regia*, the reſiduum was expoſed to the flame of a candle with tartar as before; and, by the aid of a lens, I could juſt perceive two or three metallic grains in the fuſed maſs. To this maſs the *aqua regia* was again applied, and 0,15 grain of Algaroth powder was obtained, and no other metallic calx was found but iron. A third affuſion of *aqua regia* indicated an exceedingly minute portion of Algaroth powder; but I could afterwards perceive no more metallic grains in the reſiduum expoſed to flame with tartar, nor obtain more Algaroth powder from the ſolution of the fuſed maſs in *aqua regia.* The reſiduum that reſiſted ſolution melted readily with a little phoſphoric acid into an opaque, ſomewhat yellowiſh, white globule, not unlike calcined bone fuſed with phoſphoric acid, and a minute portion of flowers of antimony. The quantity, however, of this reſiduum was ſo ſmall, that I deſpaired of determining its nature further by more experiments.

Having found that this inſoluble powder would not melt with ſulphur when heated *red-hot*, I made the following experiment.

(*d*) Twenty grains of it being mixed with three times its weight of ſulphur, were put into one of Mr. WEDGWOOD's crucibles that would contain one ounce-meaſure, which, with a cover well luted on, was put into a three-inch Engliſh crucible, and calcined bone in powder filled the ſpace between the two crucibles. After expoſing this charge to a red heat half an hour, and in a white heat ten minutes, the crucibles were cooled; and being opened, the pyrometer piece of WEDGWOOD in the bone aſhes was found to indicate 65°, and the mixture in the inner crucible had apparently been melted into a reſin-like maſs that adhered firmly to the ſides of the veſſel. Twenty-eight grains were ſcraped off, which, after digeſtion and boiling in marine acid, afforded ſix grains of Algaroth powder. A great deal of hepatic air was diſcharged during this ſolution, and very little ſulphur was left on the filter with the part not diſſolved by the marine acid. This undiſſolved part, which weighed ſix grains, was blackiſh, taſteleſs, not heavy. It was infuſible with the blow pipe, both alone, and mixed with ſulphur and tartar; but with phoſphoric acid it melted into a blackiſh ſcoria-like maſs. I could only conjecture, that this laſt part was antimonial calx, ſo far vitrified with phoſphorated lime as to be neither ſoluble nor reducible or fuſible, except with phoſphoric acid.

(*e*) By a ſimilar experiment, but with alkali of tartar twenty grains, ſulphur thirty grains, and ten grains of this inſoluble part of JAMES's Powder, a fuſed maſs was obtained that partially diſſolved in water, and afforded kermes mineral on pouring an acid into this ſolution; but a great part was inſoluble in water and acids, and ſeemed to be of the ſame nature as the ſix grains of reſiduum juſt mentioned (*d*).

I could only conclude from theſe experiments on this inſoluble matter, that it contained calx of antimony; but as to the proportion of it, and the other ſubſtance with which it is joined, I conjecture that it may be about half the quantity of the inſoluble powder; and that the other half is antimonial calx and phoſphorated lime, ſo highly calcined and vitrified together as to reſiſt ſolution in acid menſtrua, decompoſition by charcoal, and fuſion with fixed alkalies, but not by phoſphoric acid.

I ſhould not have been ſatisfied with here terminating this analyſis without enquiring further into the nature of this inſoluble matter; but I diſcontinued this analytic inveſtigation in order to derive light from the ſynthetic experiments which will be related hereafter.

Theſe laſt experiments ſeem to ſhew, that the proportion of antimonial calx is not ſo great as might have been aſſigned from the experiments with nitrous acid, marine acid, and *aqua regia*.

The ſubſtances and proportions of them, obtained from 240 grains of JAMES's Powder, by the above experiments, are as follow:

	Grains.
Phoſphorated lime, with a little antimonial calx,	100,
Algaroth powder, ...	57,15
Inſoluble antimonial calx, with a little phoſphorated lime, ...	19,85
The ſame inſoluble calx, with, probably, a little phoſphorated lime, ...	55,
Waſte, ...	8,
	240,0

As it may be objected, that concluſions drawn concerning the nature of calces might be erroneous if nitrous acid had been applied previouſly to ſubſtances containing them, I made the following experiment.

Experiment with marine acid applied to JAMES's *Powder, which had not been expoſed to the action of nitrous acid, or any other menſtruum.*

50 grains of JAMES's powder were digeſted, and gently boiled in two ounce-meaſures and a half of concentrated marine acid diluted with one ounce of diſtilled water till there remained only about one ounce-meaſure. A great part of the powder appeared to be evidently diſſolved. On cooling, cryſtals of muriated antimony were formed upon a white ſediment. The clear liquid with the cryſtals being decanted, the ſediment was boiled twice, as before, with marine acid; but the ſecond affuſion of this menſtruum brought off but eight grains of this powder, and the third only four grains. The remaining ſediment, being well dried, weighed 14 grains. Now it has been ſhewn already, that the nitrous and marine acids, ſucceſſively applied, diſſolved $\frac{177}{240}$, or all but about 60 grains; and in the preſent experiment, the marine acid diſſolved $\frac{36}{50}$, which is in the proportion of $\frac{180}{250}$, or nearly $\frac{173}{240}$; ſo that, on account of the trifling difference in theſe proportions, it may, perhaps, be fairly concluded, that the properties of the calx in JAMES's Powder are not altered by nitrous acid to affect its ſolubility in marine acid. And further, this inſoluble powder in the preſent experiment was found to have the ſame properties as that in the former experiments.

To know whether JAMES's Powder contained any ſubſtance that could be decompoſed by mild fixed alkalies, the following experiments were made.

Theſe experiments with fixed alkali ſeemed to be eſpecially neceſſary, becauſe phoſphoric acid, lime, and antimonial calx, are ingredients in JAMES's Powder; and it was obvious to ſuſpect, that this acid might be united with calx of antimony as well as with lime; which phoſphorated antimony would be decompoſed by alkalies, and yield phoſphorated alkali.

Experiments with fixed alkalies.

100 grains of JAMES's Powder were boiled in ſix ounces of water, with 50 grains of mild alkali of tartar, for three hours, and then the remaining liquid was filtered, and evaporated to dryneſs; but the matter left after evaporation was nothing but the alkali uſed in the experiment, with a little of the powder itſelf.

The reſult was the ſame on making the experiment with cryſtallized mineral alkali inſtead of alkali of tartar.

Synthetic Experiments.

ALTHOUGH the inability to prepare JAMES's Powder would not prove the above concluſions, with reſpect to its compoſition, to be erroneous; the being able to compoſe a ſubſtance poſſeſſing all the ſame properties as JAMES's Powder, by uniting or mixing together the ſubſtances ſhewn by the above analyſis to enter into its compoſition, would afford all the proof and demonſtration which can be had in the ſcience of chemiſtry.

The above analyſis ſhewed no eſſential ingredients of JAMES's Powder but antimonial calces, phoſphoric acid, and calcareous earth, which two laſt ſubſtances appeared to be united together; but it would have been vain and unneceſſary labour to have attempted to make this powder by mixtures of any of the commonly known calces of antimony and phoſphorated lime; becauſe none of them, from their well known qualities, could form a powder of the ſame colour and ſpecific gravity as JAMES's Powder, and like it partially ſoluble in acids. From the above experiments, however, the probability was evident, that this ſubſtance might be made by calcining together antimony and bone-aſhes; which operation produces a powder called LILE's and SCHAWANBERG's fever-powder; a preparation deſcribed by SCHRODER and other chemiſts 150 years ago. The receipts for this preparation differed in the proportion of the antimony to the bone aſhes, and in the ſtate of the bone; ſome directing bone ſhavings to be previouſly boiled in water; others ordered them to be burnt to aſhes before calcining them with antimony; and in other preſcriptions the bone ſhavings were directed to be burnt with the antimony. According to the receipt in the poſſeſſion of Mr. BROMFIELD, by which this powder was prepared forty-five years ago, and before any medicine was known by the name of JAMES's Powder, two pounds of hart's horn ſhavings muſt be boiled to diſſolve all the mucilage, and then, being dried, be calcined with one pound of crude antimony, till the ſmell of ſulphur ceaſes, and a light grey powder is produced. The ſame preſcription was given to Mr. WILLIS, above forty years ago, by Dr. JOHN EATON, of the College of Phyſicians, with the material addition, however, of ordering the calcined mixture to be expoſed to a great heat in a cloſe veſſel to render it *white*. Mr. TURNER made this powder above thirty years ago by calcining together equal weights of burnt hart's horn and antimony in an open veſſel, till all the ſulphur was driven off, and the mixture was of a light grey colour. He likewiſe was acquainted with the fact, that by a ſufficient degree of fire in a cloſe veſſel this cineritious powder turned *white* *. Mr. TURNER alſo prepared this powder with a pound and a half of hart's horn ſhavings and a pound of antimony, as well as with ſmaller proportions of bone. SCHRODER preſcribes equal weights of antimony and calcined hart's horn; and POTERIUS and MICHAELIS, as quoted by FREDERIC HOFFMAN, merely order the calcination of theſe two ſubſtances together (aſſigning no proportion), in a reverberatory fire for ſeveral days. In the London *Pharmacopœia* of 1788, this powder is called *Pulvis antimonialis*; and it is directed to be prepared by calcining together equal weights of hart's horn ſhavings and antimony.

Powders made from various proportions of antimony and bone-aſhes, after ſolution in nitrous acid, left a reſiduum of antimonial calx much leſs or greater in quantity than JAMES's Powder did by the ſame menſtruum, except two of Mr. TUR-

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* It is probable, that this powder was made for ſeveral years with merely the heat neceſſary to carry off the ſulphur and calcine the bone, in an open veſſel over a charcoal fire in a common grate, and conſequently it was of a light clay or aſh colour. In this manner, Mr. BROMFIELD told me, he prepared SCHAWANBERG's Powder 46 or 47 years ago. Its property of turning *white* in a greater degree of fire appears to have been a ſubſequent diſcovery.

NER's proportions, *viz.* two parts of antimony and one of calcined bone, and equal weights of bone ſhavings and antimony. The quantity of this calx was, however, greater in the powder from the former of theſe two laſt proportions than the latter of them; which latter correſponded ſometimes exactly, and always nearly, with the weight of the calx from a given weight of JAMES's Powder. This calx afforded alſo the ſame proportion of Algaroth powder as the calx in JAMES's Powder; and the inſoluble part of the calx afforded metallic grains like thoſe from the inſoluble part of the calx in that powder.

I found then an exact correſpondence between what I conſider to be the eſſential and peculiar properties of JAMES's Powder, and the properties of a powder made by uniting or mixing together the ingredients of JAMES's Powder found by analyſis. But, in order to ſhew the identity or difference of the qualities of theſe two ſubſtances, I made comparative obſervations on them, and repeated the above analytic experiments on JAMES's Powder with the preparation made by calcining together equal weights of bone ſhavings and antimony, in an open veſſel, to carry off the ſulphur, and then in cloſe veſſels applying ſuch a degree of fire as to render them *white*, that is, on the ſame preparation as the *Pulvis antimonialis* of the London *Pharmacopœia*.

Firſt, I compared, more particularly, the ſenſible qualities of ſeveral different ſpecimens of JAMES's Powder with various parcels of the *Pulvis antimonialis* made by different chemiſts. All of theſe would be called white powders, but not two of them were ſo in the ſame degree. Moſt of the papers of the *Pulvis antimonialis* were whiter than thoſe of JAMES's Powder; but others were of a very light ſtone colour, and ſome had a ſhade of yellow, ſo as to reſemble very exactly JAMES's Powder;

but all the parcels of JAMES's Powder had either a ſhade of yellow or of ſtone colour, and none were perfectly white, or ſo white as ſome ſpecimens of the *Pulvis antimonialis.* Some of the parcels of JAMES's Powder and of the *Pulvis antimonialis* taſted braſſy; and other ſpecimens of both powders had no taſte. All of theſe powders were gritty. Moſt of the parcels of the *Pulvis antimonialis* were a little ſpecifically heavier than thoſe of JAMES's Powder. The ſpecific gravity of both powders was increaſed by expoſing them to ſuch a degree of fire as brought them into almoſt a ſemi-vitrified ſtate; and, on the contrary, the ſpecific gravity of the *Pulvis antimonialis* was leſs than it is in its uſual ſtate, when made in ſuch a degree of fire that the mixture preſerves the powdery form.

The experiments with water on the *Pulvis antimonialis* produced the ſame kind of appearances, but more ſlightly than thoſe with JAMES's Powder; for the hot ſolution of the former grew leſs milky on cooling than that of the latter, and on evaporation to dryneſs leſs ſediment was found of the ſolution of *Pulvis antimonialis* than after that of JAMES's Powder*.

The experiments with acetous acid on the *Pulvis antimonialis* ſhewed, that this menſtruum diſſolved ſometimes a greater, and ſometimes a ſmaller proportion of it than of JAMES's powder; and the diſſolved matter was found to be antimonial calx, phoſphorated lime, and calx of iron, and no other ſubſtance.

It has been already ſaid, that the proportion of ſoluble matter in nitrous acid was the ſame, or nearly ſo, of the *Pulvis antimonialis* as that of JAMES's Powder; and this diſſolved matter was phoſphoric acid, calcareous earth, with a little antimonial calx, and a minute portion of calx of iron, as exactly as could be expected from the nature of the ſubſtances and the experiments, in the ſame proportion as thoſe in James's Powder.

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* The reaſon for this difference is aſſigned in another place.

The Algaroth powder, obtained by means of ſolution of the *Pulvis antimonialis* in marine acid, was in the ſame proportion as nearly as could reaſonably be expected from the nature of the experiments as that obtained from James's Powder. And the part that reſiſted ſolution in this menſtruum was partially reducible to a metallic form, and had otherwiſe the ſame properties, as far as diſcovered, as the inſoluble part of James's Powder.

Having now formed a powder poſſeſſed of properties ſimilar in *kind* to every one of thoſe aſcertained in James's Powder, with ſcarcely any difference in the *degree* of them, if it be thought that among theſe properties are thoſe which are eſſential and peculiar ones of James's Powder, the concluſion that theſe two are the ſame kind of things muſt be admitted to be juſt. The nature of one of the ingredients of James's Powder, *viz.* the irreducible part of the inſoluble matter, p. 342. is not fully elucidated by the ſynthetic experiments; but in ſo far as they ſhew, that this part equally exiſts in the powder formed by calcining together antimony and bone, which is concluded to be James's Powder, the objection againſt the concluſion with reſpect to the identity of the two ſubſtances, on the ground of this inconſiderable part of James's Powder not being well underſtood, muſt be of little weight.

Several reaſons, more intereſting to myſelf than to the Society, induced me to authenticate by additional teſtimonies thoſe analytic experiments, which may be conſidered to be more deciſive than the reſt for eſtabliſhing the identity of James's Powder, and a powder formed by calcining together antimony and bone-aſhes. I therefore requeſted Mr. Cavallo and Mr. Turner to be

preſent

preſent when I made thoſe experiments on the *Pulvis antimonialis*, prepared by Mr. GRIFFIN, of Apothecaries' Hall, and JAMES's Powder. Having, in the preſence of theſe two Gentlemen*, broken the ſeal of a phial of JAMES's Powder, bought of F. NEWBERY, and taken out of it the quantity required for the experiments, the bottle was again ſealed by Mr. CAVALLO with his ſeal, as well as the phial from which was taken the *Pulvis antimonialis*. Should any experiments be publiſhed, which eſtabliſh different concluſions from thoſe contained in this Paper, with reſpect to the identity of theſe two powders, I ſhall be happy to endeavour to aſcertain the truth by experiments, on the remaining parcels of the two powders, in the preſence of competent judges.

I ſhall next relate the experiments made with the view of confirming or invalidating the concluſions drawn from the above analyſis, with reſpect to the ingredients and proportions of them in JAMES's Powder; and by which I eſpecially endeavoured to make ſuch antimonial calces as this ſubſtance contains, by proceſſes different from thoſe above related.

EXP. I. (*a*) Hart's horn ſhavings, of ſix different parcels, well dried, ſeparately calcined in the ſame manner, and apparently to the ſame degree as when calcined with antimony to make LILE's Powder, afforded a light brown coarſe powder, with a few thin light black pieces, and loſt from 43 to 48 *per cent.* of their weight. The mean loſs of weight, of courſe, was 45½ *per cent.*

(*b*) This calcined bone (*a*), being pulverized, was expoſed to a greater degree of fire, in cloſe veſſels, than that neceſſary to render the calcined mixture of antimony and bone-aſhes *white*. The loſs of weight by this ſecond calcination or expoſure to fire was from two to three *per cent.*; and the aſhes were as white as ſnow. The total mean loſs of weight, by theſe two calcinations, was then $\frac{48}{100}$.

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* Dr. CLARKE alſo was preſent at the beginning of theſe experiments.

EXP. II. 2000 grains of coarſely powdered antimony were calcined in an earthen diſh, as in making LILE'S Powder, by conſtantly raking them about for above three hours. During a great part of this time the veſſel was red hot at the bottom; and for the laſt hour the ſulphureous fumes had entirely ceaſed. The calx thus produced was of a pale bluiſh colour; it melted, in a low degree of heat, into an opaque, ſcoria-like brittle maſs; it yielded no hepatic air with marine acid; it weighed 1409 grains, or the antimony loſt nearly 29½ *per cent.* The pyrometer in the veſſel with the antimony during its calcination, was contracted to the 6th degree of WEDGWOOD'S ſcale.

The ſum therefore of the loſs of antimony and bone by calcination in this manner, ſeparately, was 37½ *per cent.* Theſe two ſubſtances were in the next place calcined together in the ſame manner in an open veſſel, as above mentioned.

EXP. III. 2000 grains of antimony from the ſame parcel as that in the laſt experiment, and an equal weight of hart's horn ſhavings taken from the ſame parcel as thoſe were in Exp. I. were calcined together in the ſame manner that theſe ſubſtances had been ſeparately. During the firſt quarter of an hour, the mixture ſmoked, was black, ſmelled ſtrongly of ſulphur, and felt ſoft. For half an hour more, the ſmell of ſulphur continued, the mixture turned brown, and the bone was reduced to aſhes. At the end of this time, not only the bottom of the veſſel might be kept red hot without any ſigns of fuſion; but the ſmell of ſulphur, though weakly, continued for half an hour more in a heat to keep a great part of the mixture red hot. At this time the ſulphureous ſmell rather ſuddenly diſappeared, and could not be perceived, though a little of the mixture was made quite red hot for a quarter of an hour further; during which no fume was ſeen, or ſmell perceived. After cooling, a light grey or cineritious heavy powder was left; on examining which, argentine *ſpicula* were ſeen in the larger grains of this calcined ſubſtance. It weighed 2200 grains, therefore the loſs of weight was 45 *per cent.* The Wedgwood pyrometer pieces indicated 8°. In other ſimilar experiments the loſs, by calcination, was from 37 to 41 *per cent.*; therefore the mean proportion loſt in theſe experiments muſt be ſtated at 41 *per cent.*.

It appears, that the calcination of antimony with bone-aſhes is much more ſpeedy than when by itſelf, but the degree of fire was a little greater in the laſt experiment than in that with antimony alone. Conſidering the nature of theſe experiments, perhaps, it may be more reaſonable to impute the 3½ *per cent.* greater loſs in this laſt experiment than the ſum of the loſs in Exp. 1. and 2. to the greater inſenſible ſublimation of the calx from more fire in one caſe than in the other, than to refer it to the larger quantity of air combined with the metal in the former of theſe two laſt experiments.

EXP. IV. The above light clay or aſh-coloured powder, obtained in the laſt experiment by calcining together antimony and bone, being expoſed to various degrees of fire from 20° to 165° of Wedgwood's pyrometer, in cloſe crucibles, was not at all increaſed in weight, but generally loſt about 5 *per cent.* when a pretty large quantity, as a pound, was in the veſſel. A part of this loſs muſt be referred to the adheſion or vitrification of the charge with the ſides of the crucible, and part to the deficiency of the bone itſelf, as above ſhewn, by further expoſure to fire. I am ſenſible, that in experiments of this nature all calculation muſt neceſſarily, to a certain degree, be vague; yet it may be of ſome application to obſerve, that the proportion of antimonial calx, eſtimated to be contained in LILE's Powder or *Pulvis antimonialis*, and JAMES's Powder, p. 343. from the analyſis of them, does not differ more conſiderably from the proportion of this calx than may, perhaps, be reaſonably expected on calculation from theſe four laſt experiments to exiſt in them: for $70\frac{1}{2}$ parts of antimonial calx, p. 352. to $54\frac{1}{2}$ parts of bone-aſhes, p. 351. is as about 56,4 parts of this calx to 43,6 parts of calcined bone; and, on analyſis, JAMES's Powder afforded $\frac{57}{100}$ of antimonial calx, and $\frac{43}{100}$ of phoſphorated lime, or nearly ſo, p. 343. allowing for the waſte.

EXP. V. This experiment ſhews the degree of fire neceſſary to render the antimony calcined with bone of a white colour; and that this whiteneſs does not depend on the air, but on the fire.

(*a*) 1500 grains of the calcined mixture of antimony and bone, Exp. 3. were kept red hot in a cloſe veſſel for half an hour. On cooling, I found the powder changed from a cineritious or clay colour to a whitiſh colour with a ſhade of yellow. The ſides of the crucible were not glazed. The pyrometer in the middle of the powder had contracted to 40°. This powder was much inferior in whiteneſs to JAMES's Powder, being much yellower.

(*b*) Another parcel of the ſame powder, Exp. 3. was expoſed in the ſame manner, but to a greater degree of fire, in which the crucible was almoſt white hot for half an hour. After cooling, the powder was found changed to a looſely cohering, ſnow-white, heavy maſs, and the ſides of the crucible were covered with a yellow glaze. This maſs, which was eaſily detached from the veſſel, was found covered with a yellow vitreous coat over the whole ſurface of it that had been in contact with the crucible. In the white ſolid, on breaking it, many argentine *ſpicula* were ſeen. The pyrometer uſed in all theſe experiments indicated 71°.

(*c*) 1500 grains of the ſame parcel, Exp. 3. were expoſed in an open crucible to the fire of a melting furnace; no fumes aroſe till the crucible began to be almoſt white hot. After inverting another crucible, with a ſmall hole in its bottom, the fumes continued to aſcend at times through the aperture for a quarter of an hour. The crucible was then taken out of the fire, and on cooling a *whitiſh powder* was found, but no glazing, and the pyrometer indicated 28°. On again expoſing this crucible with one inverted over it in the melting furnace, but to a greater degree of fire, ſtill more fumes aroſe; but, on cooling, the charge was ſtill in the ſtate of a powder. though *whiter* than before; and the inſide of the inverted crucible was covered with ſilvery particles, and the hole of it was ſurrounded with argentine *ſpicula*, in a ſtellated form. The pyrometer indicated 39°. On reducing a little of this powder to a greater degree of fineneſs, it was as white as JAMES's Powder, with a yellowiſh caſt like it, but inferior in whiteneſs to a ſpecimen of *Pulvis antimonialis*. This crucible, containing its charge, with a cover cloſely luted on it, was put again into the fire, which was raiſed much higher than before; and, after being expoſed in it twenty minutes, the powder in the crucible became a looſely cohering ſolid, as *white as ſnow*, with a vitreous yellow coat, as before obſerved; the inſide of the crucible was glazed and covered with *ſpicula*. The pyrometer-piece in the middle of the powder was alſo covered with a yellow coat, but not glazed, and it indicated 81°. This looſely cohering ſolid being pulverized afforded a *whiter* powder than JAMES's Powder.

(*d*) The crucible, with its charge (*b*), having a cover well luted on it, was again put into the furnace, and the fire raiſed to almoſt as great a degree as I was able. This intenſe heat was kept up above an hour. After cooling, a white hard ſolid maſs was found within the crucible. On breaking the veſſel, to detach from it the charge, this ſolid maſs was found as hard as marble, and to have received its figure from the crucible. Its ſurface was covered with a yellow vitreous coat, and the whole inſide of the veſſel had a beautiful gold-coloured glaze with many argentine *ſpicula*. The pyrometer piece in the middle of the charge was alſo covered with a fine yellow glaze, and indicated 166°. This ſolid, hard maſs weighed only 21 grains leſs than before the experiment, though the whole inſide of the crucible was glazed, and had ſhining *ſpicula* upon it. A piece of this hard maſs being pulverized, it afforded a whiter powder than JAMES's powder is in general.

EXP. VI. 2000 grains of coarſely powdered antimony, mixed with 1105 grains of calcined hart's horn in powder, were calcined firſt in an open veſſel, and then expoſed to a great degree of fire in a cloſe veſſel, as in the above experiments with bone-ſhavings, Exp. 3. and 4. The calcination of this mixture in the open veſſel afforded 2550 * grains, of a leſs whitiſh and rather yellowiſh powder, inſtead of a light aſh-colour, as with bone ſhavings, Exp. 3. p. 353.; and by the ſecond, and even repeated expoſure to fire, it never could be made quite ſo white, but ſeemed more inclined to melt than the powder prepared with unburnt bone. In other reſpects the effects of fire were apparently the ſame, or nearly ſo, as in the experiments with bone ſhavings, Exp. 3, 4.; for though the loſs of weight in this experiment, reckoning that of the antimony at 29½ *per cent.*, and that of the bone aſhes at 2½ *per cent.* ſhould have left 2483 only, inſtead of 2550; yet, in other ſimilar experiments, the product correſponded nearer to this calculation, and the loſs was ſometimes leſs both of the antimony and bone calcined ſeparately. Some of the perſons who prepare the *Pulvis antimonialis* ſay, that the whiteſt colour is obtained by firſt boiling the bone ſhavings to diſſolve their mucilage, and then calcining them with antimony as above ſhewn. Mr. LILE's receipt directs previous decoction of the hart's horn.

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* In another experiment of this kind 2400 grains of antimony and 1500 grains of calcined bone afforded 3450 grains of yellowiſh light-brown powder. In a third trial, 600 grains of antimony and 400 grains of calcined bone gave 850 grains of yellowiſh brown powder.

It will not be difficult, from theſe experiments, to give a probable reaſon for the JAMES's Powder being generally of a yellowiſh caſt, and for different parcels of it, as well as of the *Pulvis antimonialis*, being generally of different degrees of whiteneſs and ſhades of yellow. The colour of this preparation is, however, a very delicate one. I once directed a perſon to calcine together antimony and bone ſhavings, in the uſual manner, to that ſtate in which the white powder may be produced by a due degree of fire; but, inſtead of a ſnow-white maſs, I could not by any degree of fire obtain any colour but a dirty whitiſh or light ſtone colour; though repeated calcinations were employed. The reaſon of the failure was, that the earthen diſh had been broken during the calcination, and a few very ſmall pieces of it had ſcaled off, and being mixed with the powder occaſioned this diſappointment with reſpect to colour. The ſame diſappointment has been alſo occaſioned by uſing a ruſty iron rod in calcining the mixture.

The bone-aſhes procured from the ſal ammoniac and ſpirit of hart's horn manufactories, frequently failed in producing a white powder; and ſo did ſometimes the bone-aſhes, called prepared hart's horn, ſold by the druggiſts. Even after a fine white coloured maſs had been made, if it was pulverized in an iron mortar that had extremely little calx upon its ſurface, or dirt, the powder was not white.

The yellow coat and glaze on the ſides of the crucible and ſurface of the calcined mixture of bone and antimony, in theſe experiments, is to be aſcribed rather to the fuſion of the clay of the crucible with the antimonial calx, than to the greater degree of fire in the part of the crucible in which it takes place; or than to the calx of iron and ſiliceous earth of the veſſel: becauſe the ſame yellow coat and glazing are produced on the WEDGWOOD pyrometer pieces, which are placed in the middle of the charge, and where the degree of heat cannot be ſo great as nearer the ſide of the crucible, and yet a ſnow-white maſs is produced between theſe clay pieces and the ſides of the crucible. This effect of clay, in forming a yellow coat and glaze, is ſhewn by the obſervation of what happens when the calcined mixture is put into a WEDGWOOD's crucible, which is made of much purer clay than other veſſels of this kind, and when it is ſet in a larger Heſſian crucible with the ſpace betwixt the two veſſels filled with the ſame calcined mixture. After expoſure to a ſufficient degree of fire, *viz.* about 120° of WEDGWOOD's ſcale, the inſide and outſide of the inner crucible will be covered with a yellow vitreous coat and glaze as well as the inſide of the outer crucible in contact with the charge, while the reſt of the matter within theſe veſſels is of a ſnowy whiteneſs. This yellow coat is one reaſon for the powder being of a ſhade of yellow in ſome ſpecimens.

Suppoſing the fuſibility of the antimonial calces to be diminiſhed the more they are calcined; the following experiment ſhews, that the antimonial calx in JAMES's Powder is more calcined than that in Exp. 2.

EXP. VII. 70½ grains of calcined antimony, as prepared in Exp. 2. triturated with 53½ grains of calcined bone, formed a powder of a bluiſh caſt, which being expoſed in a cloſe crucible, for half an hour, in a melting furnace, the degree of fire in which was 120° of WEDGWOOD's ſcale, it was found melted into a vitreous, pale bluiſh maſs; and the inſide of the crucible was glazed yellow, with red ſtreaks, and had argentine *ſpicula* adhering to it.

EXP. VIII. 800 grains of the calcined antimony of Exp. 2. were calcined for eight hours in a diſh, as in making LILE's Powder, by ſtirring it conſtantly, and keeping the bottom of the veſſel red hot during the whole time; the two laſt hours alſo the whole of the powder was kept red hot. On cooling, this calx was an impalpable light-brown powder.

(*a*) 100 grains of this calx, triturated with an equal quantity of calcined hart's horn, formed a powder very unlike JAMES's Powder, for it was of a light-brown colour. On expoſing it to about 120° of fire it melted into a yellow opaque maſs.

(*b*) The remaining 700 grains of the calcined antimony of this experiment were expoſed to fire and air as before for eight hours longer, and kept red hot a great part of the time; but the calx became very little lighter coloured than before.

(*c*) 100 grains of this calx laſt mentioned (*b*), triturated with as much calcined hart's horn, being expoſed to the degree of fire uſually applied in making the *Pulvis Antimonialis*, in a cloſe veſſel, the mixture melted partially into a greyiſh maſs.

(*d*) 150 grains of the calcined antimony (*b*) of this experiment were mixed with an equal weight of calcined hart's horn. This mixture was raked about in an earthen diſh for an hour, during a great part of which time it was red hot. On cooling, the powder was evidently lighter coloured than before this calcination. It was then expoſed in a cloſe crucible to a white heat for half an hour; and, after cooling, a looſely cohering white ſolid, with a vitreous yellow coat, was found, little inferior in whiteneſs, and otherwiſe reſembling JAMES'S Powder.

(*e*) 300 grains of the calcined antimony (*b*) of this experiment were raked about in an earthen diſh for an hour, a great part of which time they were kept red hot. On cooling, the calx was found of the ſame colour as before; and after expoſing it in a cloſe crucible in the melting furnace to almoſt a white heat for half an hour, it was obſerved to have been melted into a yellowiſh maſs.

It ſeems at leaſt very probable, from this experiment, that no degree or duration of fire, applied in open or cloſe veſſels to antimony *alone*, can produce a calx of the ſame kind as that in JAMES'S Powder: nor, perhaps, can ſuch a powder be compoſed by fire applied, in cloſe veſſels, to calx of antimony mixed with calcined bone; but if antimony duly calcined be mixed with calcined bone, and expoſed to air, in a due degree of fire, for a ſufficient length of time, and then a ſtill greater degree of fire be applied to it in cloſe veſſels, ſuch a compound may be formed as JAMES'S Powder. This experiment alſo proves, that the ſulphur in antimony is no ways neceſſary to the formation of this compound.

The manner in which air and fire act upon the antimonial calx and phoſphorated lime, I ſhall venture to conjecture.

It is probable, that the calx of antimony and phoſphorated lime combine with each other. 1. Becauſe it requires the application of heat and air for a ſhorter ſpace of time to ſeparate the ſulphur from a given quantity of antimony mixed with bone-aſhes than to produce this effect on antimony *by itſelf*: nor can the ſpeedy calcination of antimony with bone-aſhes be explained by ſuppoſing that the antimony can then bear more heat without melting; for the difference in the degree of heat applied in the two caſes is not, apparently, ſufficient to account for the difference of the times required for deſulphurating the antimony. 2. Becauſe it appears, that heat, applied to antimony in a conſiderable variety of degrees, and air for various ſpaces of time, formed a calx very different in colour, fuſibility, and other chemical qualities, from that produced by calcining this metallic ſubſtance with bone-aſhes. The ſtrongeſt confirmation, perhaps, of the opinion that the antimonial calx and phoſphorated lime are chemically united together is, that, however long the calcination of the antimony and bone-aſhes is continued in the open veſſel it will only produce preciſely the ſame ſubſtance, with reſpect to chemical properties, that is produced the moment the ſulphureous fumes ceaſe.

But why is a ſnow-white powder produced by expoſing a mixture of calcined antimony and bone-aſhes to air and fire for a due length of time, and then applying a greater degree of fire in cloſe veſſels, whereas no ſuch white powder is formed by a mixture of any calx of antimony and bone-aſhes, expoſed to any degree of fire in cloſe veſſels, without previous expoſure to fire and air? The reaſon may be, that in order that the calx ſhould unite with the phoſphorated lime, it muſt be calcined to one

certain

certain degree; which is effected by expoſure to air and fire with the bone-aſhes when it can part or combine with air, ſo as to be reduced to that ſtate in which it will be duly calcined for union with that ſubſtance, which could not happen in cloſe veſſels.

If it be objected, that this explanation does not account for the *whiteneſs* of this preparation, which is only produced by a white heat, and to which air is not neceſſary, the difficulty will be removed by conſidering that this whiteneſs may be induced without any chemical alteration effected by the fire: for, after the firſt calcination in the open veſſel, it ſeems to act, principally, in the ſame way that it does in making grey coloured bone-aſhes, or imperfectly burnt bone, of a ſnowy whiteneſs, namely, by totally deſtroying matter extraneous to the phoſphoric ſelenite. Fire alſo, in many inſtances, alters the colour of bodies without occaſioning any change in their compoſition; and, perhaps, the change of the light clay or cineritious powder, formed by the calcination of antimony and bone-aſhes in open veſſels, to a ſnowy-white ſubſtance by further expoſure to fire, depends in part upon its increaſe of ſpecific gravity or other mechanical effects of fire. A ſtriking example of the power of fire to change the colour of bodies, by merely increaſing their ſpecific gravity, is afforded by the operation of quartation, in which proceſs, the ſilver being parted, the gold is left of the colour of copper; but, by expoſure to a due degree of fire, it is changed to its well known yellow colour, without undergoing any alteration except an increaſe of ſpecific gravity.

To elucidate the nature of the inſoluble and infuſible part of JAMES's Powder, I made the following experiments, in which I particularly had in view to determine whether ſeveral antimonial calces be wholly ſoluble in acids.

EXP. IX. (*a*) Needle-like cryſtals of Algaroth powder diſſolved readily and totally in about thirty times their weight of marine acid.

(*b*) Part of the ſame parcel of cryſtallized Algaroth powder was calcined for above two hours, during which time it was expoſed to as great a heat as it would bear without melting, and during which time it was conſtantly raked about. Nearly half of this calcined calx readily diſſolved in marine acid, and by boiling the remainder in a proportionally much greater quantity of the ſame acid, great part of it was diſſolved, and the ſmall part which ſtill reſiſted ſolution could not be diſſolved in above 100 times its quantity of hot *aqua regia*. This indiſſoluble part afforded regulus with tartar by means of heat applied with the blow-pipe.

(*c*) White flowers of antimony generally left a reſiduum that was either inſoluble, or diſſolved with great difficulty, and in a ſmall proportion, in marine acid or *aqua regia*; yet this reſiduum was reducible. Some parcels of this calx *totally* diſſolved.

(*d*) A little of the antimony, long calcined in a former experiment, and afterwards melted into a yellow maſs, Exp. 8. (*a*), would only partially diſſolve in marine acid and *aqua regia*; but the copious reſiduum it left was reduced.

(*e*) Equal weights of cryſtals of Algaroth powder and calcined bone mixed together, diſſolved *totally* and readily in marine acid. This ſhews, that diſengaged phoſphoric acid does not precipitate antimonial calx when marine acid is preſent.

(*f*) The *calx antimonii nitrata* of the Edinburgh Diſpenſatory, argentine flowers of antimony, hyacinthine glaſs of antimony, and calx precipitated from antimonial tartar by alkali of tartar, all diſſolved readily and *wholly* in marine acid; but, (*g*) Diaphoretic antimony left a reſiduum which mixed with tartar formed metallic grains under the flame applied by means of the blow-pipe.

(*h*) Any of the above ſoluble antimonial calces by further calcination with air and fire become more difficultly ſoluble, or partly indiſſoluble.

The next experiments were made principally for the purpoſe of knowing whether antimony calcined with vitriolic ſelenite, calcareous earth, and ſiliceous earth, would afford the ſame ſort of calx as antimony calcined with bone-aſhes.

EXP. X. 1500 grains of well burnt and dry plaſter of Paris, mixed with as much pulverized antimony, were calcined together in the ſame manner as the mixture for making LILE's Powder, Exp. 3. In half an hour the ſulphureous fumes diſappeared; after calcining half an hour longer in a heat that kept the bottom of the diſh red hot, the mixture was of a reddiſh brown or copper colour, and after cooling weighed 2520 grains. Suppoſing, therefore, the whole deficiency of weight in this experiment to be from the ſulphur carried off; and ſuppoſing the quantity of air combined with the metal to be the ſame as in Exp. 2. the loſs of weight *viz.* 32 *per cent.* is more than would have been expected; but as in experiments of this nature it is not perhaps poſſible to repeat them under preciſely the ſame circumſtances, the difference of 2½ *per cent.* deficiency more than would have been calculated, may more reaſonably be aſcribed to the ſublimation of antimony than to other cauſes. By expoſure to 70° of fire in a cloſe crucible, this calcined mixture changed to a pale ſtraw-coloured powder, and the ſides of the veſſel were glazed yellow. The change of colour was the ſame in an open veſſel in 60° of fire.

Though it is probable, from this experiment, that there is an affinity between antimonial calx and vitriolic ſelenite, it is plain that the compound is very different from JAMES's Powder.

The next experiment with chalk and antimony, which Dr. BLAGDEN had the goodneſs to ſuggeſt, would lead to ſeveral concluſions, but I ſhall only take notice of the compoſition produced.

EXP. XI. 1200 grains of antimony were mixed with 800 grains of well waſhed, dried, and pulverized chalk, and calcined as in making LILE's Powder. In leſs than an hour the ſmell of ſulphur diſappeared; after which the mixture was calcined half an hour longer. It afforded a lighter clay-coloured powder than the calcination of antimony with bone-aſhes; and weighed 1800 grains. By expoſure to 100° of fire this powder changed to a dirty white colour. On examination, inſtead of aërated lime or chalk, there was found vitriolic ſelenite, part of which was probably combined with the antimonial calx; for, by means of boiling water repeatedly applied till the lixivium did not become turbid with muriated barytes nor acid of ſugar, I could only obtain 12 *per cent.* of vitriolic ſelenite, mixed with a little antimonial calx; but by means of nitrous acid I ſeparated 45 *per cent* of this ſelenite, with ſcarcely any antimonial calx in it. The reſiduum, after this ſolution in nitrous acid, was calx of antimony with a little vitriolic ſelenite ſeemingly vitrified. Accordingly the compoſition may be ſtated to conſiſt of 1000 parts of antimonial calx and 950 parts of vitriolic ſelenite which I infer from the quantity of ſelenite diſſolved by the nitrous acid, and eſtimated to remain united to the calx; and from the following calculation of the proportion of theſe two ingredients formed in the experiment.

Antimony.		Sulphur.		Air.		
1200	–	300	+	100	=	1000 Antimonial calx.
Calcar. earth.		Aerial acid.		Vitriolic acid.		
800	–	300	+	450	=	950 Vitriolic ſelenite.
						1950
Loſs by ſublimation and waſte						150
						1800

With regard to the nature of this calx, the greateſt part of it readily diſſolved in marine acid; and part of what then remained was alſo diſſolved, but with great difficulty and very ſparingly; a minute quantity reſiſted ſolution entirely.

EXP. XII. 600 grains of coarſely powdered antimony were mixed with 400 grains of purified white ſand, and calcined as in making LILE's Powder. The ſmell of ſulphur continued for one hour and a half, and the mixture was calcined for half an hour longer. On cooling, a brown powder was obtained which weighed 820 grains, and expoſed to 100° of fire, melted into an irregularly figured, blackiſh maſs, full of cavities.

In this experiment the loſs of weight correſponds nearly to that in experiments above related, *viz.* thoſe in which the deficiency of weight after calcining antimony alone was about 29½ *per cent.* The much longer time required in this experiment for carrying off the ſulphur than in the calcinations with bone-aſhes, gypſum, and chalk, perhaps is owing to there being no affinity between antimonial calx and ſiliceous earth.

I beg leave to mention one more experiment relative to JAMES's Powder.

EXP. XIII. A medicine is ſold by F. NEWBERY, under the title of "JAMES's Powder for Horſes, Horned Cattle, Hounds, &c." It is a light clay-coloured, gritty, taſteleſs ſubſtance, in which are ſeen ſmall *ſpicula.* It appears to me to be nothing more than JAMES's Powder for Fevers, or LILE's Powder above-mentioned, made by calcining antimony and bone-aſhes together in open veſſels; becauſe, 1ſt, by expoſure to a white heat in cloſe veſſels, it turns as white as JAMES's Powder. 2dly, It diſſolves partially in nitrous acid; and the remainder diſſolves partially in marine acid. The nitrous ſolution contains phoſphoric acid and calcareous earth; and the muriatic ſolution affords Algaroth powder.

From the whole of the above *analytical experiments* it appears:

1. That JAMES's Powder conſiſts of phoſphoric acid, lime, and antimonial calx; with a minute quantity of calx of iron, which is conſidered to be an accidental ſubſtance.

2. That either, theſe three eſſential ingredients are united with each other, forming a triple compound, or, phoſphorated lime is combined with the antimonial calx, compoſing a double compound in the proportion of about 57 parts of calx and 43 parts of phoſphorated lime.

3. That this antimonial calx is different from any other known calx of antimony in ſeveral of its chemical qualities. About three-fourths of it are ſoluble in marine acid, and afford Algaroth powder; and the remainder is not ſoluble in this menſtruum, and is apparently vitrified.

From the preceding *ſynthetic experiments* it appears, that by calcining together bone-aſhes, that is, phoſphorated lime, and antimony in a certain proportion, and afterwards expoſing the mixture to a white heat, a compound was formed conſiſting of antimonial calx and phoſphorated lime, in the ſame proportion, and poſſeſſing the ſame kind of chemical properties, as JAMES's Powder.

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XXII. *An Account of ſome chemical Experiments on* Tabaſheer. *By* James Louis Macie, *Eſq. F. R. S.*

Read July 7, 1791.

THE Tabaſheer employed in theſe experiments was that which Dr. RUSSELL laid before the Society, as ſpecimens of this ſubſtance, the evening his Paper upon the ſubject was read *.

There were ſeven parcels.

N° 1. conſiſted of Tabaſheer extracted from the bamboo by Dr. RUSSELL himſelf.

N° 2. had been partly taken from the reed in Dr. RUSSELL's preſence, and partly brought to him at different times by a perſon who worked in bamboos.

N° 3. was the Tabaſheer from Hydrabad; the fineſt kind of this ſubſtance to be bought.

N^os 4, 5, and 6. all came from Maſulapatam, where they are ſold at a very low price. Theſe three kinds have been thought to be artificial compoſitions in imitation of the true Tabaſheer, and to be made of calcined bones.

N° 7. had no account affixed to it.

The Tabaſheer from Hydrabad being in the greateſt quantity, and appearing the moſt homogeneous and pure, the experiments were begun, and principally made, with it.

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* See Phil. Tranſ. Vol. LXXX. p. 283.

Hydrabad Tabaſheer. (N° 3.)

§ I. (A) This, in its general appearance, very much reſembled fragments of that variety of calcedony which is known to mineralogiſts by the name of *Cacholong*. Some pieces were quite opaque, and abſolutely white; but others poſſeſſed a ſmall degree of tranſparency, and had a bluiſh caſt. The latter, held before a lighted candle, appeared very pellucid, and of a flame colour.

The pieces were of various ſizes; the largeſt of them did not exceed two or three tenths of an inch cubic. Their ſhape was quite irregular; ſome of them bore impreſſions of the inner part of the bamboo againſt which they were formed.

(B) This Tabaſheer could not be broken by preſſure between the fingers; but by the teeth it was eaſily reduced to powder. On firſt chewing it felt gritty, but ſoon ground to impalpable particles.

(C) Applied to the tongue, it adhered to it by capillary attraction.

(D) It had a diſagreeable earthy taſte, ſomething like that of magneſia.

(E) No light was produced either by cutting it with a knife, or by rubbing two pieces of it together, in the dark; but a bit of this ſubſtance, being laid on a hot iron, ſoon appeared ſurrounded with a feeble luminous *auréole*. By being made red hot, it was deprived of this property of ſhining when gently heated; but recovered it again, on being kept for two months.

(F) Examined with the microſcope, it did not appear different from what it does to the naked eye.

(G) A quantity of this Tabaſheer which weighed 75.7 gr. in air, weighed only 41.1 gr. in diſtilled water whoſe temperature was 52.5 F. which makes its ſpecific gravity to be very nearly = 2.188.

Mr. CAVENDISH, having tried this ſame parcel when become again quite dry, found its ſpecific gravity to be = 2.169.

Treated with water.

§ II. (A) This Tabaſheer, put into water, emitted a number of bubbles of air; the white opaque bits became tranſparent in a ſmall degree only, but the bluiſh ones nearly as much ſo as glaſs. In this ſtate the different colour produced by reflected and by tranſmitted light was very ſenſible.

(B) Four bits of this ſubſtance, weighing together, while dry and opaque, 4.1 gr., were put into diſtilled water, and let become tranſparent; being then taken out, and the unabſorbed water haſtily wiped from their ſurface, they were again weighed, and were found to equal 8.2 gr.

In the experiment § I. (G), 75.7 gr. of this ſubſtance abſorbed 69.5 gr. of diſtilled water.

(C) Four bits of Tabaſheer, weighing together 3.2 gr. were boiled for 30′ in half an ounce of diſtilled water in a Florence flaſk, which had been previouſly rinced with ſome of the ſame fluid. This water, when become cold, did not ſhew any change on the admixture of vitriolic acid, of acid of ſugar, nor of ſolutions of nitre of ſilver, or of cryſtals of ſoda; yet, on its evaporation, it left a white film on the glaſs, which could not be got off by waſhing in cold water, nor by hot marine acid; but which was diſcharged by warm cauſtic vegetable alkali, and by long ebullition in water.

Upon theſe bits of Tabaſheer, another half ounce of diſtilled water was poured, and again boiled for about half an hour. This water alſo on evaporation left a white film on the glaſs veſſel, ſimilar to the above. The pieces of Tabaſheer having been dried, by expoſure to the air for ſome days in a warm room, were found to have loſt one-tenth of a grain of their weight.

To aſcertain whether the whole of a piece of Tabaſheer could be diſſolved by boiling in water, a little bit of this ſubſtance, weighing three-tenths of a grain, was boiled in 36 ounces of ſoft water for near five hours conſecutively; but being afterwards dried and weighed, it was not diminiſhed in quantity, nor was it deprived of its taſte.

With vegetable colours.

§ III. Some Tabaſheer, reduced to fine powder, was boiled for a conſiderable time in infuſions of turnſole, of logwood, and of dried red cabbage, but produced not the leaſt change in any one of them.

At the fire.

§ IV. (A) A piece of this Tabaſheer, thrown into a red hot crucible, did not burn or grow black. Kept red hot for ſome time, it underwent no viſible change; but when cold, it was harder, and had entirely loſt its taſte. Put into water it grew tranſparent, juſt as it would have done, had it not been ignited.

(B) 6.4 gr. of this ſubſtance, made red hot in a crucible, were found, upon being weighed as ſoon as cold, to have loſt two-tenths of a grain. This loſs appears to have ariſen merely from the expulſion of interpoſed moiſture; for theſe heated pieces, on being expoſed to the air for ſome days, recovered exactly their former weight.

(C) A bit of this ſubſtance was put into an earthen crucible, ſurrounded with ſand, and kept red hot for ſome time; when cold, it was ſtill white, both exteriorly and interiorly.

(D) Thrown into ſome melted red hot nitre, this ſubſtance did not produce any deflagration, or ſeem to ſuffer any alteration.

(E) A bit expoſed on charcoal to the flame of the blow-pipe did not decrepitate or change colour; when firſt heated it diffuſed a pleaſant ſmell; then contracted very conſiderably in bulk, and became tranſparent; but on continuing the heat it again grew white and opaque, but ſeemed not to ſhew any inclination to melt *per ſe*. Poſſibly, however, it may ſuffer ſuch a ſemi-fuſion, or ſoftening of the whole maſs, as takes place in clay when expoſed to an intenſe heat; for when the bit uſed happened to have cracks, it ſeparated during its contraction, at theſe cracks, and the parts receded from each other without falling aſunder.

If, while the bit of Tabaſheer was expoſed to the flame, any of the aſhes of the coal fell upon it, it inſtantly melted, and ſmall very fluid bubbles were produced. That the opacity which this ſubſtance acquires on continuing to heat it after it is become tranſparent, is not owing to the fuſion of its ſurface by means of ſome of the aſhes of the charcoal ſettling upon it unobſerved, appeared by its undergoing the ſame change when fixed to the end of a glaſs tube, in the method of M. DE SAUSSURE *.

With acids.

§ V. (A) A piece of Tabaſheer, weighing 1.2 gr. was firſt let ſatiate itſelf with diſtilled water; its ſurface being then wiped dry, it was put into a matraſs with ſome pure white marine acid, whoſe ſpecific gravity was 1.13. No efferveſcence aroſe on its immerſion into the acid; nor did this menſtruum, even by ebullition, ſeem to have any action upon it, or itſelf receive any colour. The acid being evaporated left only ſome dark coloured ſpots on the glaſs. Theſe ſpots were diſſolved by diſtilled water. No precipitation was produced in this water by vitriolic acid, or by a ſolution of cryſtals of ſoda. The bit of Tabaſheer waſhed with water, and made red hot, had not ſuſtained any loſs of weight.

The pores of the maſs of Tabaſheer were filled with water before it was put into the acid, to expel the common air contained in them, and which would have made it impoſſible to aſcertain with accuracy whether any efferveſcence was produced on its firſt contact with the menſtruum.

(B) Another portion of Tabaſheer, weighing 10.2 gr. was boiled in ſome of the ſame marine acid. Not the leaſt precipitate was produced on ſaturating this acid with ſolution of mild ſoda. This Tabaſheer alſo, after having been

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* Journal de Phyſique, Tom. XXVI. p. 409.

boiled

grew tranſparent, juſt as it would have done, had it not been ignited.

(B) 6.4 gr. of this ſubſtance, made red hot in a crucible, were found, upon being weighed as ſoon as cold, to have loſt two-tenths of a grain. This loſs appears to have ariſen merely from the expulſion of interpoſed moiſture; for theſe heated pieces, on being expoſed to the air for ſome days, recovered exactly their former weight.

(C) A bit of this ſubſtance was put into an earthen crucible, ſurrounded with ſand, and kept red hot for ſome time; when cold, it was ſtill white, both exteriorly and interiorly.

(D) Thrown into ſome melted red hot nitre, this ſubſtance did not produce any deflagration, or ſeem to ſuffer any alteration.

(E) A bit expoſed on charcoal to the flame of the blow-pipe did not decrepitate or change colour; when firſt heated it diffuſed a pleaſant ſmell; then contracted very conſiderably in bulk, and became tranſparent; but on continuing the heat it again grew white and opaque, but ſeemed not to ſhew any inclination to melt *per ſe*. Poſſibly, however, it may ſuffer ſuch a ſemi-fuſion, or ſoftening of the whole maſs, as takes place in clay when expoſed to an intenſe heat; for when the bit uſed happened to have cracks, it ſeparated during its contraction, at theſe cracks, and the parts receded from each other without falling aſunder.

If, while the bit of Tabaſheer was expoſed to the flame, any of the aſhes of the coal fell upon it, it inſtantly melted, and ſmall very fluid bubbles were produced. That the opacity which this ſubſtance acquires on continuing to heat it after it is become tranſparent, is not owing to the fuſion of its urface by means of ſome of the aſhes of the charcoal ſettling upon it unobſerved, appeared by its undergoing the ſame change when fixed to the end of a glaſs tube, in the method of M. DE SAUSSURE *.

With acids.

§ V. (A) A piece of Tabaſheer, weighing 1.2 gr. was firſt let ſatiate itſelf with diſtilled water; its ſurface being then wiped dry, it was put into a matraſs with ſome pure white marine acid, whoſe ſpecific gravity was 1.13. No efferveſcence aroſe on its immerſion into the acid; nor did this menſtruum, even by ebullition, ſeem to have any action upon it, or itſelf receive any colour. The acid being evaporated left only ſome dark coloured ſpots on the glaſs. Theſe ſpots were diſſolved by diſtilled water. No precipitation was produced in this water by vitriolic acid, or by a ſolution of cryſtals of ſoda. The bit of Tabaſheer waſhed with water, and made red hot, had not ſuſtained any loſs of weight.

The pores of the maſs of Tabaſheer were filled with water before it was put into the acid, to expel the common air contained in them, and which would have made it impoſſible to aſcertain with accuracy whether any effervеſcence was produced on its firſt contact with the menſtruum.

(B) Another portion of Tabaſheer, weighing 10.2 gr. was boiled in ſome of the ſame marine acid. Not the leaſt precipitate was produced on ſaturating this acid with ſolution of mild ſoda. This Tabaſheer alſo, after having been boiled in water, and dried by expoſure for ſome days to the air, was ſtill of its former weight.

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* Journal de Phyſique, Tom. XXVI. p. 409.

§ VI. This ſubſtance ſeemed in like manner to reſiſt the action of pure white nitrous acid boiled upon it.

§ VII. (A) A bit of Tabaſheer weighing 0.6 gr. was digeſted in ſome ſtrong white vitriolic acid, which had been made perfectly pure by diſtillation. It did not ſeem by this treatment to ſuffer any change, and after having been freed from all adhering vitriolic acid by boiling in water, it had not undergone any alteration either in its weight or properties. The vitriolic acid afforded no precipitate on being ſaturated with ſoda.

(B) Two grains of Tabaſheer reduced to fine powder were made into a paſte with ſome of this ſame vitriolic acid, and this mixture was heated till nearly dry; it was then digeſted in diſtilled water. This water, being filtered, taſted ſlightly acid, did not produce the leaſt turbidneſs with ſolution of ſoda, and ſome of it, evaporated, left only a faint black ſtain on the glaſs, produced doubtleſs by the action of the vitriolic acid on a little vegetable matter, which it had received either from the Tabaſheer, or from the paper. The undiſſolved matter collected, waſhed, and dried, weighed 1.9 gr.

§ VIII. 2 gr. of Tabaſheer, reduced to fine powder, were long digeſted in a conſiderable quantity of liquid acid of ſugar. The taſte of the liquor was not altered; and being ſaturated with a ſolution of cryſtals of ſoda in diſtilled water, it did not afford any precipitate. The Tabaſheer having been freed from all adhering acid, by very careful ablution with diſtilled water, and let dry in the air, was totally unchanged in its appearance, and weighed 1.98 gr. This Tabaſheer being gradually heated till red hot, did not become in the leaſt black, or loſe much of its weight, a proof that no acid of ſugar had fixed in it.

With liquid alkalies.

§ IX. (A) Some liquid cauſtic vegetable alkali being heated in a phial, Tabaſheer was added to it, which diſſolved very readily, and in conſiderable quantity. When the alkali would not take up any more, it was ſet by to cool, but was not found next morning to have cryſtallized, or undergone any change, though it had become very concentrated, during the boiling, by the evaporation of much of the water.

(B) This ſolution had an alkaline taſte, but ſeemingly with little, if any, cauſticity.

(C) A drop of it changed to green a watery tincture of dried red cabbage.

(D) Some of this ſolution was expoſed in a ſhallow glaſs to ſpontaneous evaporation in a warm room. At the end of a day or two it was converted into a firm, milky jelly. After a few days more, this jelly was become whiter, more opaque, and had dried and cracked into ſeveral pieces, and finally it became quite dry, and curled up and ſeparated from the glaſs.

The ſame change took place when the ſolution had been diluted with ſeveral times its bulk of diſtilled water, only the jelly was much thinner, and dried into a white powder.

Some of this ſolution, kept for many weeks in a bottle cloſely ſtopped, did not become a jelly, or undergo any change.

(E) A ſmall quantity of this ſolution was let fall into a proportionably large quantity of ſpirit of wine, whoſe ſpecific gravity was .838. The mixture immediately became turbid, and, on ſtanding, a denſe fluid ſettled to the bottom, and which, when the bottle was haſtily inverted, fell through the ſpirit of wine in round drops, like a ponderous oil.

The ſupernatant ſpirit of wine being carefully decanted off, ſome diſtilled water was added to this thick fluid, by which it was wholly diſſolved. This ſolution, expoſed to the air, ſhewed phænomena exactly ſimilar to thoſe of the undiluted ſolution (D).

The decanted ſpirit being alſo left expoſed to the air in a ſhallow glaſs veſſel, did not, after many days, either depoſit a ſenſible quantity of precipitate, or become gelatinous; but having evaporated nearly away, left a few drops of a liquor which made infuſion of red cabbage green; and, on the addition of ſome pure marine acid, efferveſced violently. No precipitate fell during this ſaturation with the acid; nor did the mixture on ſtanding become a jelly; and on the total evaporation of the fluid part, a ſmall quantity of muriate of tartar only remained. The ſpirit of wine ſeems, therefore, to have diſſolved merely a portion of ſuperabundant alkali preſent in the mixture, but none of that united with Tabaſheer.

(F) To different portions of this ſolution were added ſome pure marine acid, ſome pure white vitriolic acid, and ſome diſtilled vinegar, each in exceſs. Theſe acids at firſt produced neither heat, efferveſcence, any precipitate, or the leaſt ſenſible effect, except the vitriolic acid, which threw down a very ſmall quantity of a white matter; but, after ſtanding ſome days, theſe mixtures changed into jellies ſo firm, that the glaſſes containing them were inverted without their falling out.

This change into jelly equally took place whether the mixtures were kept in open or cloſed veſſels, were expoſed to the light, or ſecluded from it; nor did it ſeem to be much promoted by boiling the mixtures.

(G) Some ſolution of mild volatile alkali in diſtilled water, being added to ſome of this ſolution, ſeemed at the firſt inſtant of mixture to have no effect upon it; but in the ſpace of a ſecond or two it occaſioned a copious white precipitate.

(H) The flakes remaining on the glaſſes at (D) and (E) put into marine acid raiſed a ſlight effervеſcence, but did not diſſolve. Theſe flakes, when taken out of the acid, and well waſhed, were found, like the original Tabaſheer, to be white and opaque when dry; but to become tranſparent when moiſtened, and then to ſhew the blue and flame colour, § II. (A).

(I) The jellies (F), diluted with water, and collected on a filter, appeared to be the Tabaſheer unchanged.

§ X. A bit of Tabaſheer, weighing two-tenths of a grain, was boiled in 127 gr. of ſtrong cauſtic volatile alkali for a conſiderable time; but after being made red hot, it had not ſuſtained the leaſt diminution of weight.

§ XI. (A) 27 gr. of Tabaſheer, reduced to fine powder, were put into an open tin veſſel with 100 gr. of cryſtals of ſoda, and ſome diſtilled water, and this mixture was made boil for three hours. The clear liquor was then poured off, and the Tabaſheer was digeſted in ſome pure marine acid; after ſome time this acid was decanted, and the Tabaſheer waſhed with diſtilled water, which was then added to the acid.

(B) This Tabaſheer was put back into the alkaline ſolution, which ſeemed not impaired by the foregoing proceſs, and again boiled for a conſiderable time. The liquor was then poured from it while hot, and the Tabaſheer edulcorated with ſome cold diſtilled water, which was afterwards mixed with this hot ſolution, in which it inſtantly cauſed a precipitation. On heating the mixture it became clear again; but as it cooled it changed wholly into a thin jelly; but, in the courſe of a few days, it ſeparated into two portions, the jelly ſettling in a denſer ſtate to the bottom of the veſſel, leaving a limpid liquor over it.

(C) The Tabaſheer remaining (B) was boiled in pure marine acid; the acid was then poured off, and the Tabaſheer edulcorated with ſome diſtilled water, which was afterwards mixed with the acid.

(D) The remaining Tabaſheer collected, waſhed, and dried, weighed 24 gr. and ſeemed not to be altered.

(E) The acid liquors (A and C) were mixed together, and ſaturated with ſoda, but afforded no precipitate.

(F) The alkaline mixture (B) was poured upon a filter, the clear liquor came through, leaving the jelly on the paper.

Some of this clear liquor, expoſed to the air in a ſaucer, at the end of ſome days depoſited a ſmall quantity of a gelatinous matter; after ſome days more, the whole fluid part exhaled, and the ſaucer became covered with regular cryſtals of ſoda, which afforded no precipitate during their ſolution in vitriolic acid. What had appeared like a jelly while moiſt aſſumed, on drying, the form of a white powder. This powder was inſoluble in vitriolic acid, and ſeemed ſtill to be Tabaſheer.

Some of this clear liquor, mixed with marine acid, effervesced; did not afford any precipitate; but, on ſtanding ſome days, the mixture became ſlightly gelatinous.

(G) Some of the thick jelly remaining on the filter, being boiled in water and in marine acid, appeared inſoluble in both, and ſeemed to agree entirely with the above powder (F).

With dry alkalies.

§ XII. (A) Tabaſheer melted on the charcoal at the blowpipe with ſoda, with conſiderable efferveſcence. When the proportion of alkali was large, the Tabaſheer quickly diſſolved, and the whole ſpread on the coal, ſoaked into it, and vaniſhed; but, by adding the alkali to the bit of Tabaſheer in exceedingly ſmall quantities at a time, this ſubſtance was converted into a pearl of clear colourleſs glaſs.

(B) 5 gr. of Tabaſheer, reduced to fine powder, were melted in a platina crucible with 100 gr. of cryſtals of ſoda. The maſs obtained was white and opaque, and weighed 40.2 gr. Put into an ounce of diſtilled water, it wholly diſſolved. An exceſs of marine acid let fall into this ſolution produced an efferveſcence, and changed it into a jelly. This mixture was ſtirred about, and then thrown upon a filter. The jelly left on the paper did not diſſolve in marine acid by ebullition; collected, waſhed with diſtilled water, and dried, it weighed 4.5 gr. and ſeemed to be the Tabaſheer unaltered.

The liquor which had come through being ſaturated with mineral alkali yielded only a very ſmall quantity of a red precipitate, which was the colouring matter of the pink blotting paper through which it had been paſſed.

(C) 10 gr. of Tabaſheer, reduced to powder, were mixed with an equal weight of ſoda, deprived of its water of cryſtallization by heat. This mixture was put into a platina crucible, and expoſed to a ſtrong fire for 15′. It was then found converted into a tranſparent glaſs of a ſlight yellow colour. This glaſs was broken into pieces, and boiled in marine acid. No effervefſcence appeared; but the glaſs was diſſolved into a jelly. This jelly, collected on a filter, well waſhed, and dried, weighed 7.7 gr.

The acid liquor which came through, on ſaturation with ſoda, afforded not the leaſt precipitate; but, after ſtanding a day or two, it changed into a thin jelly. This collected on a filter was waſhed with diſtilled water, and then boiled in marine acid, but did not diſſolve. Being again edulcorated, and made red hot, it weighed 1.6 gr. The filtered liquor (B) would in all probability have changed ſimilarly to a jelly, had it been kept. Theſe precipitates were analogous to thoſe § IX. (I).

(D) An equal weight of vegetable alkali and Tabaſheer were melted together in the platina crucible. The glaſs produced was tranſparent; but it had a fiery taſte, and ſoon attracted the moiſture of the air, and diſſolved into a thick liquor. But two parts of vegetable alkali, with three of Tabaſheer, yielded a tranſparent glaſs, which was permanent.

Treated with other fluxes.

§ XIII. (A) A fragment of Tabaſheer put into glaſs of borax, and urged at the blow-pipe, contracted very conſiderably in ſize, the ſame as when heated *per ſe*; after which it continued turning about in the flux, diſſolving with great difficulty and very ſlowly. When the ſolution was effected, the ſaline pearl remained perfectly clear and colourleſs.

(B) With phoſphoric ammoniac (made by ſaturating the acid obtained by the ſlow combuſtion of phoſphorus with cauſtic volatile alkali) the Tabaſheer very readily melted on the charcoal at the blow-pipe, with effervеſcence, into a white frothy bead.

(C) Fuſed, by the ſame means, on a plate of platina, with the vitriols of tartar and ſoda, it appeared entirely to reſiſt their action; the little particles employed continuing to revolve in the fluid globules without ſuſtaining any ſenſible diminution of ſize, and the ſaline beads on cooling aſſumed their uſual opacity.

(D) A bit of Tabaſheer was laid on a plate of ſilver, and a little litharge was put over it, and then melted with the blow-pipe. It immediately acted on the Tabaſheer, and covered it with a white glaſſy glazing. By the addition of more litharge the maſs was brought to a round bead, though with conſiderable difficulty. This bead bore melting on the charcoal, without any reduction of the lead, but could not be obtained tranſparent.

(E) The eaſe with which this ſubſtance had melted with vegetable aſhes, led to the trial of it with pure calcareous earth. A fragment of Tabaſheer, fixed to the end of a bit of glaſs, was rubbed over with ſome powdered whiting. As ſoon as expoſed to the flame of the blow-pipe, it melted with conſiderable effervеſcence; but could not, even on the charcoal, and with the addition of more whiting, be brought to a tranſparent ſtate, or reduced into a round bead.

Equal weights of Tabaſheer and pure calcareous ſpar, both reduced to fine powder, were irregularly mixed, and expoſed in the platina crucible to a ſtrong fire in a forge for 20′; but did not even concrete together.

(F) When magneſia was uſed, no fuſion took place at the blow-pipe.

(G) Equal parts of Tabaſheer, whiting, and earth of alum precipitated by mild volatile alkali, were mixed in a ſtate of powder, and ſubmitted in the platina crucible to a ſtrong fire for 20′, but were afterwards found unmelted.

Examination of the other ſpecimens.

N° I.

This parcel contained particles of three kinds; ſome white, of a ſmooth texture, much reſembling the foregoing ſort; others of the ſame appearance, but yellowiſh; and others greatly ſimilar to bits of dried mould.

The white and yellowiſh pieces were ſo ſoft as to be very eaſily rubbed to powder between the fingers. They had a diſagreeable taſte, ſomething like that of rhubarb. Put into water, the white bits ſcarcely grew at all tranſparent; but the yellow ones became ſo to a conſiderable degree.

The brown earth-like pieces were harder than the above, had little taſte, floated upon water, and remained opaque.

Expoſed to the blow-pipe, they all charred and grew black; the laſt variety even burned with a flame. When the vegetable matter was conſumed, the pieces remained white, and then had exactly the appearance, and poſſeſſed all the properties, of the the foregoing Tabaſheer from Hydrabad, and like it melted with ſoda into a tranſparent glaſs.

N° II.

Alſo conſiſted of bits of three ſorts.

(*a*) Some white, nearly opaque.

(*b*) A few ſmall very tranſparent particles, ſhewing, in an eminent degree, the blue and yellow colour, by the different direction of light.

(*c*) Coarſe, browniſh pieces of a grained texture.

Theſe all had exactly the ſame taſte, hardneſs, &c. and ſhewed the ſame effects at the blow-pipe, as N° I.

27 gr. of this Tabaſheer thrown into a red-hot crucible, burned with a yellowiſh white flame, loſt 2.9 gr. in weight, and became ſo ſimilar to the Hydrabad kind as not to be diſtinguiſhed from it.

Some of this Tabaſheer put into a crucible, not made very hot, emitted a ſmell ſomething like tobacco aſhes, but not the kind of perfume diſcovered in that from Hydrabad, § IV. (E).

N° IV.

All the pieces of this parcel were of one appearance, and a good deal reſembled, in their texture, the third variety of N° II. Their colour was white; their hardneſs ſuch as very difficultly to be broken by preſſure between the fingers. In the mouth they immediately fell to a pulpy powder, and had no taſte.

A bit expoſed on the charcoal to the blow-pipe became black, melted like ſome vegetable matters, caught flame, and burnt to a botryoid inflated coal, which ſoon entirely conſumed away, and vaniſhed.

A piece put into water fell to a powder. The mixture being boiled, this powder diſſolved, and turned the whole to a jelly.

Theſe properties are exactly thoſe of common ſtarch.

N° V.

Agreed entirely with N° IV. in appearance, properties, and nature.

N° VI.

The pieces of this parcel were white, quite opaque, and conſiderably hard. Their taſte and effects at the blow-pipe, were perfectly ſimilar to thoſe of the Hydrabad kind.

N° VII.

Much reſembled N° VI. only was rather ſofter, and ſeemed to blacken a little when firſt heated. With fluxes at the blow-pipe it ſhewed the ſame effects as all the above.

Concluſion.

1. It appears from theſe experiments, that all the parcels, except N° IV. and V. conſiſted of genuine Tabaſheer; but that thoſe kinds, immediately taken from the plant, contained a certain portion of a vegetable matter, which was wanting in the ſpecimens procured from the ſhops, and which had probably been deprived of this admixture by calcination, of which operation a partial blackneſs, obſervable on ſome of the pieces of N° III. and VI. are doubtleſs the traces. This accounts alſo for the ſuperior hardneſs and diminiſhed taſtes of theſe ſorts.

2. The nature of this ſubſtance is very different from what might have been expected in the product of a vegetable. Its indeſtructibility by fire; its total reſiſtance to acids; its uniting

by fuſion with alkalies in certain proportions into a white opaque maſs, in others into a tranſparent permanent glaſs; and its being again ſeparable from theſe compounds, entirely unchanged by acids, &c. ſeem to afford the ſtrongeſt reaſons to conſider it as perfectly identical with common *ſiliceous earth*.

Yet from pure quartz it may be thought to differ in ſome material particulars; ſuch as in its fuſing with calcareous earth, in ſome of its effects with liquid alkalies, in its taſte, and its ſpecific gravity.

But its taſte may ariſe merely from its divided ſtate, for chalk and powdery magneſia both have taſtes, and taſtes which are very ſimilar to that of pure Tabaſheer; but when theſe earths are taken in the denſer ſtate of cryſtals, they are found to be quite inſipid; ſo Tabaſheer, when made more ſolid by expoſure to a pretty ſtrong heat, is no longer perceived, when chewed, to act upon the palate, § IV. (A).

And, on accurate compariſon, its effects with liquid alkalies have not appeared peculiar; for though it was found on trial, that the powder of common flints, when boiled in ſome of the ſame liquid cauſtic alkali employed at § IX. (A), was ſcarcely at all acted upon; and that the very little which was diſſolved was ſoon precipitated again, in the form of minute *flocculi*, on expoſing the ſolution to the air, and was immediately thrown down on the admixture of an acid; yet the precipitate obtained from *liquor ſilicum* by marine acid was diſcovered, even when dry to diſſolve readily in this alkali, but while ſtill moiſt to do ſo very copiouſly, even without the aſſiſtance of heat; and ſome of this ſolution, thus ſaturated with ſiliceous matter by ebullition, being expoſed to the air in a ſhallow glaſs, became a jelly by the next day, and the day after dried, and cracked, &c. exactly like the mixtures § IX.

(D and E). And another portion of this ſolution mixed with marine acid afforded no precipitate, and remained perfectly unaffected for two days; but on the third it was converted into a firm jelly like that § IX. (F).

As gypſum is found to melt *per ſe* at the blow-pipe, though refractory to the ſtrongeſt heat that can be made in a furnace, it was thought that poſſibly ſiliceous and calcareous earths might flux together by this means, though they reſiſt the utmoſt power of common fires; but experiment ſhewed, that in this reſpect quartz did not agree with Tabaſheer. But this difference ſeems much too likely to depend on the admixture of a little foreign matter in the latter body, to admit of its being made the grounds for conſidering it as a new ſubſtance, in oppoſition to ſo many more material points in which it agrees with ſilex.

Nor can much weight be laid on the inferior ſpecific gravity of a body ſo very porous. The infuſibility of the mixture § XIII. (G) depended alſo, probably, either on an inaccuracy in the proportions of the earths to each other, or on a deficiency of heat.

3. Of the three bamboos which were not ſplit before the Royal Society I have opened two. The Tabaſheer found in them agreed entirely in its properties with that of N° I. and II.

It was obſerved, that all the Tabaſheer in the ſame joint was exactly of the ſame appearance. In one joint it was all ſimilar to the yellowiſh ſort N° I. In another joint of the ſame bamboo, it reſembled the variety (*c*) of N° II. Probably, therefore, the parcels from Dr. RUSSELL, containing each ſeveral varieties of this ſubſtance, aroſe from the produce of many joints having been mixed together.

4. The aſhes, obtained by burning the bamboo, boiled in marine acid, left a very large quantity of a whitiſh inſoluble powder, which, fuſed at the blow-pipe with ſoda, efferveſced, and formed a tranſparent glaſs. Only the middle part of the joints was burned, the knots were ſawed off, leſt, being porous, Tabaſheer might be mechanically lodged in them. However, the great quantity of this remaining ſubſtance ſhews it to be an eſſential, conſtituent part of the wood.

The aſhes of common charcoal, digeſted in marine acid, left in the ſame manner an inſoluble reſiduum which fuſed with ſoda with efferveſcence, and formed glaſs; but the proportion of this matter to the aſhes was greatly leſs than in the foregoing caſe.

5. Since the above experiments were made, a ſingular circumſtance has preſented itſelf. A green bamboo, cut in the hot-houſe of Dr. PITCAIRN, at Iſlington, was judged to contain Tabaſheer in one of its joints, from a rattling noiſe diſcoverable on ſhaking it; but being ſplit by Sir JOSEPH BANKS, it was found to contain, not ordinary Tabaſheer, but a ſolid pebble, about the ſize of half a pea.

Externally this pebble was of an irregular rounded form, of a dark-brown or black colour. Internally it was reddiſh-brown, of a cloſe dull texture, much like ſome martial ſiliceous ſtones. In one corner there were ſhining particles, which appeared to be cryſtals, but too minute to be diſtinguiſhed even with the microſcope.

This ſubſtance was ſo hard as to cut glaſs!

A fragment of it expoſed to the blow-pipe on the charcoal did not grow white, contract in ſize, melt, or undergo any change. Put into borax it did not diſſolve, but loſt its colour,

and

and tinged the flux green. With ſoda it effervefced, and formed a round bead of opaque black glaſs.

Theſe two beads, digeſted in ſome perfectly pure and white marine acid, only partially diſſolved, and tinged this menſtruum of a greeniſh yellow colour; and from this ſolution Pruſſite of tartar, ſo pure as not, under many hours, to produce a blue colour with the above pure marine acid, inſtantly threw down a very copious Pruſſian blue.

P. S. In aſcertaining the ſpecific gravity of the Hydrabad Tabaſheer, § I. (G), great care was taken in both the experiments that every bit was thoroughly penetrated with the water, and tranſparent to its very center, before its weight in the water was determined.

XXIII. *A Second Paper on Hygrometry.*

By J. A. De Luc, *Eſq. F. R. S.*

Read July 14, 1791.

PART II.

IN the firſt *part* of this Paper * I have treated of the fundamental principles of hygrometry, and of ſome hygroſcopic phænomena; and this will relate to a particular application of thoſe premiſes.

62. Since the publication of my firſt hygrometer, many others have been invented, two of which are now principally in uſe; the *hair* hygrometer of M. de Saussure, and my *hygrometer* made of a *ſlip of whalebone*. If the comparative points of thoſe inſtruments could be determined in the whole extent of their ſcales, the only inconvenience of their being both uſed would be, the neceſſity of reducing to one of them, the obſervations made with the other; but from 70 to 100 of mine, which ſpace includes the moſt important period of *moiſture*, their correſpondent indications are as different from one another, and as variable, as if they were the effects of two very different cauſes. Therefore it is important to decide which of them ſhould remain our only meaſure of *moiſture*, till, if poſſible, a better one is found. The following pages, I hope, will lead to that deciſion.

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* See Page 1. of this Volume.

63. The fundamental proceſs of M. DE SAUSSURE, with the view of diſcovering the effects of *moiſture* on the *hair hygrometer*, was this. He repeatedly cauſed ſucceſſive known quantities of *water* to evaporate into a cloſe glaſs veſſel, previouſly reduced to *extreme dryneſs*, and containing that *hygrometer* and a *manometer*; he obſerved the correſpondent changes of thoſe inſtruments, and, by combining the reſults of his experiments, he reduced to regular *ſeries* the correſpondent motions of the two inſtruments by equal quantities of *evaporated water*. Having confined himſelf to that only claſs of experiments, which, from cauſes that I ſhall explain, could not diſcover to him the difficulties of his attempt, he thought himſelf warranted to draw from them the following concluſions. 1ſt, That the degrees of *moiſture* in the incloſed *medium*, were nearly proportional to the quantities of *water* evaporated in the veſſel; and that, conſequently, the *ratio* obſerved between thoſe quantities and the *march* of his *hygrometer*, could be conſidered as giving immediately the *march* of the inſtrument correſpondent to *moiſture* itſelf; which, according to our common opinion, is a certain quantity of *aqueous vapours* ſpread in the *medium*. 2dly, That when no more *water* could *evaporate* in the veſſel, the incloſed *medium* was arrived at *extreme moiſture*; and that, conſequently, the point indicated at that time on his *hygrometer*, was to be the *limit* of its *ſcale* on that ſide. 3dly, That having, from thoſe experiments, a probable determination of the *expanſions* of the *hair* by ſucceſſive equal quantities of *moiſture*, in beginning from the point were this is null, and ending at its *extreme*, his inſtrument could not differ eſſentially from an abſolute *hygrometer*.

64. Theſe concluſions were very natural in the ſtate of M. DE SAUSSURE's experiments; but before their publication I had

gone over a great field of *hygroſcopic* phænomena, in which the *hair*, and a *cloſe veſſel*, had a ſhare; and thereby, ſeeing the objects in another light than M. DE SAUSSURE, I doubted of his concluſions. and I procured three of his *hygrometers*, in order to examine them on ſome particular points. It was after that immediate verification of my conjectures concerning his inſtrument, that I ſettled the following concluſions, very different from thoſe above. 1ſt, That MOISTURE, or the quantity of *vapour* ſpread in the *medium* itſelf, does not increaſe in an incloſed ſpace in proportion to the quantity of water evaporated in it; becauſe of an increaſing, but undetermined, part of that *water* being depoſited on the ſides of the veſſel; and that, conſequently, Mr. DE SAUSSURE's experiments could not afford the determination of a real *hygroſcopic-ſcale*. 2dly, That the circumſtance conſidered by him as a ſure ſign of *extreme moiſture* exiſting in the incloſed *medium*, namely, the *maximum* of *evaporation* in the ſpace, has only that effect when the *temperature* is very little above 32°; but that, by ſucceſſive increaſes of *heat* from that point, *moiſture* recedes farther and farther from its *extreme*; or from the point where no more *vapour* can be introduced in the *medium* without an immediate *precipitation*; though at the ſame time, there are ſucceſſive increaſes in the quantity of *vapour*, and thereby a conſtant *maximum* of *evaporation* correſpondent with the actual *temperature*. 3dly, That, in approaching to *extreme moiſture*, the *hair* hygrometer becomes *ſtationary*, and afterwards a little *retrograde*, in which *march* the unavoidable irregularities of every *hygroſcopic* ſubſtance produce frequent anomalies; from which cauſe it was very difficult for M. DE SAUSSURE, conſidering the form of his experiments, to diſcover the *hygroſcopic law* expreſſed by the ſecond concluſion; and with the unknown exiſtence of that *law*, to ſuſpect the *march* of his *hygrometer:* which accidental complication I ſhall explain hereafter.

65. When I publiſhed thoſe reſults of my experiments and obſervations, M. DE SAUSSURE rejected them; not from having made new experiments that had confirmed his opinions; but becauſe he conjectured inverſely, that my theory reſulted from a fallacious *march* of my *hygrometer:* and the well-earned reputation of that celebrated philoſopher engaged me to undertake every experiment that could help me to detect on which ſide was the error. I have related, in the firſt *part* of this Paper, ſome of thoſe experiments; and now, for their application, as well as for giving an account of ſome others, I ſhall follow more particularly M. DE SAUSSURE's proceſs.

66. In a large glaſs veſſel, containing (as I have mentioned above) a *manometer* and his *hair*-hygrometer, which veſſel he had previouſly reduced to a known ſmall diſtance from *extreme dryneſs*, M. DE SAUSSURE introduced from time to time a piece of *wet* cloth, which he weighed both before he put it into the veſſel, and when he took it out. The ſucceſſive increaſes in the quantity of *vapour* reſulting from that proceſs were indicated; on the *manometer*, by ſucceſſive increaſes in the quantity of the incloſed *elaſtic fluids*, which cauſed the quickſilver to aſcend more and more in that inſtrument; and on the *hygrometer*, by ſucceſſive expanſions of the *hair*. The *maximum* of *evaporation* was clearly indicated by the *manometer*; for, during every laſting *temperature*, the quickſilver, after having aſcended to a certain point, remained fixed at that point, notwithſtanding a longer ſtay of the *wet* cloth; and by repeating that operation at different *temperatures*, M. DE SAUSSURE determined the quantities of *evaporated water* that, in a given ſpace, and by a given *temperature*, produced the *maximum* of *evaporation*.

That ſingle determination, from its great conſequences not yet generally attended to, would be ſufficient to fix the celebrity of its author, as I have already expreſſed many times in other works.

67. But with reſpect to a different concluſion from the ſame experiments, no leſs important to natural philoſophy, I have alſo ſaid, that M. DE SAUSSURE's *hygrometer* may have miſled him. We have ſeen, that the *manometer* indicated, by an indubitable ſymptom, the *maximum* of *evaporation*; for here the immediate cauſe is clearly known, namely, the *quantity* of *elaſtic fluids*; and it is evident, that the quickſilver muſt aſcend in the inſtrument in proportion to that *quantity*, and ſtop when it ceaſes to increaſe. But it is not the ſame as to the indications of the *hygrometer* in reſpect of *moiſture*: M. DE SAUSSURE found himſelf that they were far from proportional to the intenſities of their cauſe; and in the laſt ſtage of his experiments, though theſe indications did not vary much by the different *maxima* of *evaporation*, they however varied in the ſpace of 1 or 2 *degrees*. But as thoſe ſmall differences on the point where the *hygrometer* ſtopped in different experiments, did not follow apparently any law conformable to the *temperature*, M. DE SAUSSURE conſidered them as ſmall *anomalies*, unavoidable in hygroſcopic ſubſtances, and of little conſequence on a ſcale of 100 *degrees*; therefore, laying aſide that circumſtance, he could have no doubt, that, in every *temperature*, the *maximum* of *evaporation* in a cloſe ſpace was ſynonymous with the *maximum* of *moiſture* in that ſpace; while, from my experiments, theſe two ſuppoſed identical expreſſions may differ $\frac{1}{5}$, and ſometimes $\frac{1}{4}$, of the real ſcale of *moiſture*, which is the caſe in the *temperature* of only 75° or 80°.

68. Let us now ſuppoſe for a moment, that the above *hygroſcopic law*, and the *march* that I attribute to the *hair* hygrometer, are real. In that caſe, if, during a conſtant *maximum* of *evaporation*, the *temperature* varies from 32° to 80°, *moiſture* will diminiſh $\frac{1}{5}$ and even $\frac{1}{4}$ of the whole; or, in other words, the ſtate of the *medium* will be diſtant by ſo much from that in which a new introduction of *vapour* would be followed by a *precipitation*. But at the ſame time, in the whole of that period of *moiſture*, the *hair* hygrometer is ſuppoſed to move only 1 or 2 *degrees* backwards and forwards, with frequent irregularities. Therefore, in the hypotheſis, ſuch a great change of *moiſture* would be hardly ſuſpected from thoſe ſmall deviations of the *hair* hygrometer, in which at firſt nothing appears to be regular; and thereby it is evident, that, by confining himſelf to thoſe experiments, M. DE SAUSSURE could not diſcover thoſe two important laws of *hygrology* and *hygrometry*, of which I have here only ſuppoſed the exiſtence.

69. Let us ſuppoſe again, that the *hair* hygrometer had not exiſted before a certain number of other experiments; and that M. DE SAUSSURE, in his attempt to produce an inſtrument of that kind, had fixed on any of the *ſlips* made of *fibrous* vegetable or animal ſubſtances cut *acroſs* the *fibres*, of which many *hygroſcopes* had been made before in a coarſer manner; and that, in every other reſpect, he had proceeded as he has done with the *hair*. In that caſe, having placed his *hygrometer* and the *manometer* in the ſame veſſel, with a quantity of *water* ſufficient for producing the *maximum* of *evaporation* in every common *temperature*, and obſerved alſo the points where both inſtruments ſtopped in different laſting *temperatures*, he then would have found; that the *hygrometer* indicated leſs and leſs *moiſture*, at the ſame time that the *manometer*, by aſcending

more and more as the *evaporation* increaſed by more *heat*, indicated that increaſe in the quantity of *vapour*, diſtinct from the expanſions of the *fluids*; and that each of thoſe inſtruments remained alſo fixed in that ſtate of oppoſite changes, by every ſufficiently durable change of *temperature*. The firſt obſervation of that phænomenon would have ſurprized him, as it did me, and it would have induced him alſo to try ſome other of thoſe *ſlips*; and by finding the ſame phænomenon with every one of them, he would have been cōnvinced, that it was a real *law* of *moiſture*. Laſtly, if he had known Mr. JAMES WATT's obſervations on the great *dryneſs* of the *ſteam* of *boiling water*, as long as it remains in a ſpace as *warm* as the *water* that produces it (a condition always underſtood when the general laws of *evaporation* are the object of inquiry); admitting with me, as it follows from his own theory, that ſuch *ſteam* is no other than the ſame kind of *vapour* thus far mentioned, only rendered capable of a greater *denſity* by more *heat*; he would then have ſeen the importance of that *hygroſcopic law* from its great extent. I have hardly any doubt, that *extreme dryneſs* would reign in a cloſe place ſimilar to PAPIN's *digeſtor*, if there were a ſufficient ſpace above the *water* and a *red heat*; though that ſpace would be filled with *vapour* to the *maximum*. This relates only to the *hygroſcopic law* here in view, on which, in the ſuppoſed caſe, I do not think we could have had any controverſy.

70. Laſtly, let us ſuppoſe, that in order to try the effect of *moiſture* on the ſubſtance he had firſt choſen, or on ſome other of the ſame kind of ſubſtances, but taken *lengthwiſe*, M. DE SAUSSURE had happened to try firſt one of thoſe which, uſed in that manner, have a great *retrogradation*, as *gooſe-quill* and *deal*; and that, after having previouſly obſerved it in the

open air, he had incloſed it in the *moiſt veſſel*, at a time when the place where it ſtood before had the degree of *moiſture* corresponding with the *ſtationary* ſtate of that *hygroſcope*, he would then have obſerved a phænomenon as little expected as the former: for after that inſtrument had been incloſed in the *moiſt veſſel*, it would have moved, by that increaſe of *moiſture*, in the ſame direction as it had done in the open *dry* air when there was on the contrary an increaſe of *dryneſs*. Surpriſed no doubt at that phænomenon, M. DE SAUSSURE would have ſubmitted his new inſtrument to more experiments; he would alſo have tried other *threads*, in which he would have found the ſame ſort of *march*, only at various degrees; and if, in the courſe of thoſe trials, he had ſubmitted the *hair* to the ſame experiments, the ſmallneſs of its motions backwards and forwards, and their irregularities, would not have prevented him from diſcovering in it the ſame ſort of *march* as he had then been uſed to ſee in other *threads*; and thereby, he would have abandoned the whole tribe of *threads* as unfit for the *hygrometer*.

71. The whole of that ſuppoſed courſe of experiments with *ſlips* and *threads*, is that which I have followed from the time I had abandoned the conſtruction of my firſt *hygrometer*; which I did eſpecially with the view of being able to try many ſubſtances. Therefore my theory was formed in conſequence of the two above concluſions, which appear to me immediate, and ſuch as M. DE SAUSSURE could not have drawn differently, if he had followed the ſame neceſſary ſteps: and now I will prove, moreover, that if it had not been for *accidental* circumſtances in his own proceſs, the *hair* alone would have engaged him by degrees to undertake the ſame experiments.

72. M. DE SAUSSURE's firſt *hygrometers*, having their *index* at the top of the frame, could be plunged into *water*; and he

tried that method for fixing their point of *extreme moiſture*, as I had done for my firſt *hygrometers*. But in thoſe trials he obſerved, that while his inſtruments ſtood in *water*, their indications remained undetermined within a ſpace of four or five degrees; and attributing that irregularity to a *friction* of the *hair* with *water*, he thought it neceſſary to change my method, to that of placing the *inſtrument* in a *moiſt medium*, which he produced by means of a glaſs jar, *wet* on the inſide, and inverted over *water*. In this method, the ſituation of the *index* became indifferent; and, for ſome particular reaſon, he placed it at the bottom of his new *hygrometers*, which then could not be plunged into *water*. This laſt circumſtance was merely accidental; however, we ſhall ſee how much it has influenced his opinions in reſpect of *extreme moiſture*.

73. In the firſt account of his experiments, M. de Saussure did not enter into thoſe particulars; but they are in his anſwer to me: and when I there ſaw, that he gave as a reaſon for having *abandoned the immerſion in water*, that a *ſtrong adheſion* of the *hair* to that liquid impeded the freeneſs of its motions, I found it more natural to aſſign to the inſtrument itſelf the unſteadineſs of its *index* which he attributed to that cauſe. Indeed, in thoſe firſt *hygrometers*, one end of the *axis* paſſed through a hole for the purpoſe of carrying the *index* outwards, which was a cauſe of much friction: that *axis* beſides was loaded with the weight of pretty large pincers, holding the *hair*, and that weight was counterpoiſed on the other ſide. Laſtly, the connexion of the *hair* with the *index* was produced by a *ſilver lamina*, which, though very thin, oppoſed ſome reſiſtance in bending round the *axis*. Thoſe are defects that M. de Saussure corrected afterwards; but they exiſted in the inſtruments which he plunged into *water*, and a weight of

only 3 *grains* was not ſufficient to keep their *index* ſteady, either in *water* or any where; and this he obſerved himſelf.

74. Notwithſtanding that natural explanation of the unſteadineſs of M. DE SAUSSURE's firſt *hygrometer* when in *water*, I thought it neceſſary to try, in the ſame circumſtance, ſome well-conſtructed *hair hygrometer*; therefore I made two, ſimilar to the laſt of M. DE SAUSSURE's in every reſpect, except that of having their *index* at the top; and for the connection of the *hair* with the *axis*, I uſed a kind of *pincers*, not above half a *grain* in weight, with a hair-like bit of *hemp*, which I know does not alter in any ſenſible manner the march of the inſtrument. Now, theſe *hair* hygrometers, with their *weight* of only 3 *grains*, being put into *water*, follow in it their own laws, arrive and remain *fixed* at their once ſettled point, as well as any of my other hygrometers. This M. DE SAUSSURE would have alſo found, if in the improvement of his inſtrument the *axis* had remained at the top.

75. Before I explain the influence which that *accidental* circumſtance of the place of his *index* has had on his opinions with reſpect to *extreme moiſture*, I muſt mention another of the ſame kind which has contributed to the ſame effect. It is evident, both from theory and from M. DE SAUSSURE's own experiments related above (§ 66.), that a ſufficient quantity of *water* in any part of a cloſe veſſel is the only requiſite for producing in it the *maximum* of *evaporation*; but, with a view of accelerating that effect for the common purpoſe of fixing the point of *extreme moiſture* on his *hygrometer*, he preſcribes *wetting* the inſide of the veſſel, beſides inverting it over *water*. He did not (and indeed he could not) foreſee the conſequence of that alteration in his firſt proceſs; but in fact it was ſuch as to prevent him from diſcovering, even with

time, in thoſe operations, what he would have ſeen immediately if his laſt *hygrometers* could have been plunged into *water*.

76. After the improvement of his *hygrometers*, their *index* was no more ſubject to that unſteadineſs obſerved in the firſt; conſequently they remained *fixed* under the *moiſt veſſel*; but they did not *fix* at the ſame point every time; and it happened *accidentally*, from cauſes of irregularity in the *veſſel* itſelf, that in ſome of the caſes, when the *hair* was the *longeſt*, a *precipitation* of *water* happened (by ſome partial cooling) on ſome part of that veſſel; and he took that appearance for a ſure ſign that there was a *ſuperfluous* quantity of *water* in the incloſed air. From that *accidental* connexion of circumſtances he concluded, that his *hygrometer* indicated two different ſtates of the *medium* in reſpect of *extreme moiſture*; one, correſpondent to about 98 on that inſtrument, which he conſidered as real *extreme moiſture*, or that ſtate of the *medium* in which no more *vapour* could be introduced into it without a *precipitation*; the other, when ſuch a *precipitation* took place, which he made correſpondent to 100, or to the greateſt *length* of the *hair*. Prepoſſeſſed afterwards with that opinion, when I publiſhed my comparative experiments of his *hygrometer* and mine, in which the latter ſtood ſometimes at 80 when the former was at 98, he concluded from that circumſtance, that while his *hygrometer* moved only 2 *degrees* by the utmoſt effect of a *ſuperſaturation* of the *medium*, mine had 20 of thoſe *inſignificant degrees*. In this centers the whole of our diſagreement, and I am now going to trace its cauſe in the *accidental* circumſtances above deſcribed.

77. When I plunge my *hair* hygrometers into *water*, where, as I have ſaid, they come to a *fixed* point, that point does not indicate the *greateſt length* of the *hair*; for, on the contrary,

that *thread* is then *ſhorter* than it is moſt times under the *moiſt veſſel.* This M. de Saussure would have ſeen, if he had not been prevented, by the ſituation of the *index* in his improved *hygrometer*, from trying again the effect of *water* on the *hair*; and that phænomenon alone would ſurely have given a different courſe to his ideas; eſpecially he would not have ſuppoſed, that the *hair* lengthens 2 *degrees* more, by a *ſuper-ſaturation* of the *medium*, or by the immediate contact of concrete *water*.

78. If, alſo, when he ſettled the manner of determining the point of *extreme moiſture* on his *hygrometers*, M. de Saussure had retained the ſimplicity of the proceſs he had uſed for his fundamental experiments, in which a piece of *wet* cloth had been ſufficient for producing the *maximum* of *evaporation* in his large veſſel; and, in conſequence, had contented himſelf with inverting his glaſs jar over *water*, without *wetting* it on the inſide, he would have avoided a great cauſe of deception which I am going to explain. In my firſt experiments on the comparative *marches* of our *hygrometers*, in which I followed M. de Saussure's preſcription for the *moiſt veſſel*, I found ſome *anomalies* which puzzled me. M. de Saussure himſelf took notice of them in the account I gave of thoſe experiments, and attributed them to my inſtrument. I did not agree with him in that reſpect; but it was long before I could diſcover the real cauſe of thoſe anomalies. The firſt ſtep towards that diſcovery, was the reflecting on the uſeleſſneſs of *wetting* the veſſel on the inſide, for the only purpoſe of producing in it the *maximum* of *evaporation.* That conſideration engaged me to undertake a new courſe of the ſame experiments, with a glaſs jar merely inverted over *water*; and by that means, the greateſt part of the real *anomalies* being removed, I obſerved clearly in the *march* of the *hair*, the

combined effects of its own property, and of the *hygroſcopic law* reſpecting *evaporation* which was known to me from other phænomena.

79. As for the cauſe of thoſe *anomalies* which had been removed by the change of the proceſs, the following phænomenon led me to diſcover it. I obſerved frequently, at times when my *hygrometer*, placed under the jar, ſtood at a conſiderable diſtance from its point of *extreme moiſture*, that a very ſmall diminution of heat was ſufficient to cauſe, on the lower part of the veſſel, the formation of a *tarniſhed* rim, extending one or two inches above the ſurface of the *water*, with a thin vaniſhing edge. Having reflected on that phænomenon, from the mechaniſm I aſſign to the operation of *fire* in the very act of *evaporation* I concluded, that in ſuch a *ſtagnant air* every *evaporating* ſurface had an *atmoſphere* of *extreme moiſture*, which extended as indicated by the *tarniſhed rim*; and that it was only beyond that limit that reigned the other *law*, of a decreaſing *moiſture*, correſpondent to the increaſing *maxima* of *evaporation* by an increaſing *heat*. That new *law* of *evaporation* offered evidently an adequate cauſe for explaining the anomalies obſerved in the *wet* veſſel; for *glaſs* retains concrete *water* very imperfectly, and it runs down very ſoon from many of the places which had retained it. Conſequently, under ſuch a partially *wet* veſſel, and differently ſo at different times, the inſtruments muſt be variouſly affected by ſcattered *atmoſpheres* of *extreme moiſture*.

80. However, before I could truſt that explanation, I wanted to ſubmit it to ſome direct experiment; and I ſucceeded by a means which at the ſame time realized what M. DE SAUSSURE thought he had obtained, namely, to produce *extreme moiſture* in a cloſe ſpace, during any common *temperature*,

without

without any *precipitation* of *water* from the *medium*. This I have produced by means of a wire cage, 4 inches in diameter, covered with cotton cloth, having at the top a *reſervoir*, by which the cloth is kept thoroughly *wet* for a long time; which cage beſides is incloſed in the glaſs jar inverted over water. In that apparatus, though in ſummer-time, every *hygrometer*, either *thread* or *ſlip*, moves and fixes itſelf, not ſo ſpeedily, but elſe exactly as if it was plunged into *water*, without any *ſuperſaturation* of the incloſed *medium*, or *precipitation* of *water* on the *hygroſcopic* ſubſtance.

81. We may ſee now that the idea of two ſorts of *extreme moiſture* is without any foundation. In order to enforce the neceſſity of taking the point of *extreme moiſture* in the *air* brought to that ſtate, and not in *water*, M. DE SAUSSURE ſays, "That the *hygrometer* is not to meaſure the *moiſture* of *water*, "but that of the *air*." This at firſt appears plauſible; however, in reality, *moiſture* is no more to be conſidered in *water* itſelf, than *heat* in the fluid called *fire*. *Water* is the cauſe of *moiſture*, as *fire* is the cauſe of *heat*; but thoſe *effects* are not produced *on* their cauſes; it is *on* other ſubſtances. Therefore, if ſome *hygroſcopic* ſubſtances are placed in a *medium* which has attained *extreme moiſture*, and, in proportion as they take *water* from it, the loſs of that *water* be conſtantly repaired by a new *evaporation*, they will receive by degrees in ſuch a *medium*, without any *precipitation*, as much *water* as if they were plunged in *water* itſelf; for the limit is their *capacity*, which I have explained in § 19. This is the ſame theory that I had expreſſed in my firſt Paper on *Hygrometry*; and it is completely confirmed by the above experiment, with the diſcovery of this new *hygroſcopic law*: "That in a *ſtagnant air*, every "*evaporating* ſurface has an *atmoſphere* of *extreme moiſture*,

"which extends in a ſpace of few inches, diminiſhes rapidly, "and does not interfere beyond that limit with the other *laws* "of *moiſture*."

82. I ſhall now explain, by an example, what is the fundamental *deviation* of an *hair hygrometer*, introducing in it, for a moment, thoſe two ſingular points 98 and 100, which, in M. DE SAUSSURE's experiments, were a very natural cauſe of miſtake. A *hair* hygrometer and mine, being in a cloſe veſſel, at a time when the *temperature*, ſenſibly conſtant, ſhall be but little above 32; if *moiſture* is firſt introduced into that veſſel, ſo as to bring the *hair* hygrometer, by a very ſlow direct motion, to 98, my *hygrometer* will ſtop between 70 and 75; and both inſtruments will be *fixed*, if *moiſture* and *heat* remain the ſame. Let *moiſture* then be made to increaſe very ſlowly, till the *hair* hygrometer has attained its point 100; mine will have arrived at 80; and they again will remain at thoſe points as long as, with the ſame *temperature*, the ſame quantity of *vapour* ſhall remain in the veſſel. Laſtly, let a ſufficient or ſuperfluous quantity of *water* be introduced into the veſſel, the *hair* hygrometer will *retrograde* to 98, and mine proceed to 100, at which points they will ſtop, whatever be the quantity of *water*; and they will remain fixed, as long as the *heat* ſhall not increaſe. This explains the riddle of the ſingular point 98, or of a certain *point*, various in different *hair* hygrometers, various even, at different times, in the ſame individual, at which that inſtrument ſtands with very different degrees of *moiſture*; conſequently, its little motions round that *point* may create great deception, eſpecially from the nature of *organized* ſubſtances, on which now I ſhall add a few words.

83. The above is the fundamental *march* of the *hair* hygrometer, ſuch as it is on the whole, and as it would be conſtantly,

ſtantly, if no other cauſe interfered; but it is ſubject to diſturbing *anomalies*, which become worthy of attention within that ſmall critical ſpace which I have deſcribed. The *texture* of organized *hygroſcopic* ſubſtances occaſions a *friction* between their parts, when, by the changes of *moiſture* and *heat* combined with their *elaſticity*, they undergo changes in their reſpective poſitions; whereby they hardly can completely return to the very ſame arrangement, though with the ſame external circumſtances; even at *extreme moiſture*, when, there being leſs *friction* between their particles, the greateſt part of the diſturbances produced in their former motions are reſtored. To that general cauſe of irregularity is added a particular cauſe, when thoſe ſubſtances are in the ſtate of *hygrometers*; this is the influence of two oppoſite forces acting conſtantly on them; one, the tendency of their component parts to remain united; the other, a *weight* or *ſpring* which tends to ſeparate them. Certain accidental arrangements of their component parts give them more power to reſiſt the action which tends to ſeparate them; and thoſe arrangements are very changeable, by the alternate introduction and expulſion of *moiſture*, by a long ſtay within a ſmall compaſs of variations, and by more or leſs *heat*. This is a large field of facts and ſpeculations, not uninteresting in itſelf, but on which I muſt not dwell: what I have ſaid of thoſe cauſes is ſufficient to account for the *anomalies* to which, more or leſs, every *hygrometer* is ſubject. But whereas in the *ſlips*, thoſe *anomalies* create only ſome irregularities in the obſervations, without any deceiving conſequence in reſpect of the *laws* of *moiſture*, they may deceive when they happen to interfere in the critical part of the march of ſome *threads*; for inſtance, if, by a certain accidental arrangement of the conſtituent parts in a *ſlip* of *whalebone*, there happens to be ſome *tenths* of a *degree* difference, from one experiment to another, on its point of *extreme moiſture*, that *anomaly* cannot be of any conſequence on the determination of what muſt be conſidered as that ſtate in the medium; but if it happens to the *hair*, which, in approaching *extreme moiſture*, has but very ſmall motions, it may reverſe thoſe which it had had naturally (as I have obſerved it ſometimes) and become a cauſe of deception.

84. I have now explained, how mere *accidental* circumſtances have been the cauſe of a difference in the ideas that M. DE SAUSSURE and I had formed on what is to be underſtood by *extreme moiſture* in every caſe; and I am going to illuſtrate the whole of that ſubject by a ſingular fact. An *hygrometer* made with a *box thread*, or a thin *faſcicle* of the fibres of that wood, being placed in open air, next to a *hair* hygrometer, or to moſt of the other inſtruments of that ſort, moves in a contrary way from them; but we may lay aſide that circumſtance, by ſuppoſing, that the *numbers*, marked on the *dial* of the firſt, are increaſing in the oppoſite direction from thoſe of the other inſtruments. Let us then ſuppoſe, that ſome experimental philoſopher had choſen the *box thread* for his *hygrometer*; with him I ſhould have fallen into no controverſy on the point of *extreme moiſture*; for, either under the *moiſt veſſel*, or in any other caſe approaching *extreme moiſture*, his *hygrometer* would have moved like mine. But the *box thread*, at approaching *extreme dryneſs*, firſt relents much its pace, then becomes *ſtationary*, and afterwards *retrograde*; by which property, with the concourſe of ſome *accidental* circumſtances as have happened in M. DE SAUSSURE's experiments, the very ſame queſtions that I have examined with ſo much labour in reſpect of *extreme moiſture*, only becauſe of the *hair* hygrometer, would have been tranſported to the point of *ex-*

treme dryneſs; which, however, till now, has created no doubt.

85. That ſingular *thread* illuſtrates alſo another point, cloſely connected with the *retrogradation* of its tribe, but not to be confounded with it; I mean the *recoil*. The common cauſe of both phænomena is, two oppoſite *effects* produced by changes of *moiſture*, on the length of *threads*. The *retrogradation* in the *march*, comparatively to that of *moiſture*, is produced by one of the *effects*, which before was ſurpaſſed by the other, becoming predominant; and the *recoil*, or a returning back a part of a firſt ſtride when *moiſture* changes ſuddenly, is produced, by one of the *effects*, that on the *fibres* themſelves, being performed *ſooner* than that on the ſort of *reticle* formed by the *fibres*. Now, the *box thread* having its *retrogradation* at approaching *extreme dryneſs*, there alſo the *recoil* becomes ſenſible: it appears in the firſt modifications of that *thread* when placed in my *dry veſſel*, by motions backwards and forwards, as it happens to ſome other *threads*, when taken out of *water*, or expoſed to any other ſudden change of *moiſture*. The *box thread* having a ſlow motion, no *recoil* is clearly diſtinguiſhable in its common *march*; whereas the *hair* and the *quill-thread*, which, in appearance, are very quick, have generally a very diſturbed motion when *moiſture* changes ſuddenly. I have ſeen them, when in a free air, and happening to be in their *ſtationary* ſtate, moving quickly one way in a ſpace of 1 or 2 degrees, and then *recoiling* ſlower, ſometimes to the ſame point where they were before, while my *hygrometer* underwent a ſteady change, which was in the firſt direction of theirs. This phænomenon, of a complete *recoil* in the *ſtationary* ſtate of quick *threads*, is ſimilar to the *recoil* of the *index* in thoſe *glaſs frames* that I have deſcribed in § 59. of this Paper, which have a

compenſation for the changes of *heat* by a thin *braſs lamina*: this being ſooner affected than the *glaſs rods*, by ſudden changes of *heat*, the *index* moves firſt one way; then it *recoils* completely, by the change being operated later in the *glaſs rods*.

86. The experiments briefly related in this Paper will, I hope, be ſufficient to anſwer the following queſtion, which has been made to me by ſome obſervers of the two principal *hygrometers* here compared: "Why does the *hair* hygrometer, "when expoſed to the open air in day time, come ſo often "near its point of *extreme moiſture*, while the *whalebone* hygro"meter almoſt never comes within 30 *degrees* diſtance of that "point in ſummer, and very ſeldom within 20 in winter, even "in *rainy* weather (if preſerved from rain)?" The anſwer, according to the reſults of theſe experiments, is this: "The "general *march* of the *hair* hygrometer is much *decreaſing*, "comparatively to equal increaſes of *moiſture*; that *march* for"wards, ends in a *ſtationary* ſtate, and is followed by a ſmall "*retrogradation*; while the *whalebone* hygrometer has con"ſtantly a *march*, if not *proportional*, at leaſt conſtantly *ſimilar* "to that of *moiſture* itſelf."

87. There remains an object of inquiry, which is, a determination of thoſe *ratios* here generally expreſſed. I have explained, in the firſt *part* of this Paper, the difficulties of that object, and what help may be found in comparing the *marches* of *hygrometers* with the *acquiſitions* of *weight* of their ſubſtances, of which proceſs I gave ſome *examples*; and here I ſhall relate ſimilar experiments on *hair*, *whalebone*, *box*, and *aloes-pitta*. But as I have already deſcribed the whole proceſs, and the manner of calculation of that claſs of experiments, I ſhall only give here the reſults of theſe laſt.

88. I muſt, however, firſt explain another *reduction* that I have added to the former. In the firſt TABLE which I have given of thoſe experiments, I followed the immediate diviſion of my inſtruments, in which o correſponds to *extreme dryneſs*, and 100 to *extreme moiſture*. But firſt, under that form, the point called 100 by M. DE SAUSSURE would not appear in its true light, as it means the greateſt *length* of the *hair*; while the point 100 of my ſcale deſigns the ſtate of that *thread* in *water*, where it has a *little retrogradation*. Under that form alſo moſt part of the terms in the obſervation on the *box thread* would be *negative*, ſince it moves very long in a contrary direction to the other hygroſcopes. For theſe reaſons, inſtead of calling o the point of *extreme dryneſs*, and that of *extreme moiſture* 100, I have, in the following TABLES, applied the firſt of thoſe denominations to the *ſmalleſt length* of each ſubſtance, and the laſt to their *greateſt length*. That *reduction* produces no difference in the *proportions* between the *terms*, and none in the *terms* themſelves in reſpect to *ſlips*; as in theſe, the *greateſt length* is always obſerved by *extreme moiſture*, and the *ſmalleſt* by *extreme dryneſs*.

II. TABLE *of comparative changes in the* weight *and in the* length *of the ſame ſubſtances, by the ſame* increaſes of moiſture, *correſpondent to the march of the* ſlip of whalebone *from 5 to 5 of its degrees.*

	WHALEBONE.		HAIR.		ALOES-PITTA.	
	Increaſes of the weight in ſhavings.	March of the ſlip.	Increaſes of the weight in a maſs of hair.	March of the hair.	Increaſes of the weight in a maſs of pitta.	March of the thread of pitta.
Extr. dryneſs	0.0	0	0.0	0.0	0.0	0.0
	6.0	5	4.8	15.7	6.0	20.6
	11.8	10	8.8	29.0	11.8	35.1
	17.3	15	12.5	40.0	17.3	51.6
	22.2	20	15.9	50.4	22.2	57.6
	26.8	25	19.1	59.7	26.8	75.6
	31.2	30	22.2	67.5	31.2	71.9
	35.2	35	26.6	74.4	35.2	76.3
	39.7	40	29.0	79.3	39.7	83.0
	44.0	45	32.0	83.3	44.0	86.6
	48.1	50	35.0	88.0	48.1	93.6
	52.1	55	38.2	90.0	52.1	96.5
	57.1	60	43.3	92.8	57.1	94.7
	61.7	65	49.8	94.1	61.7	98.2
	66.3	70	55.3	95.4	66.3	100.0
	71.9	75	61.9	97.0	71.9	99.2
	77.6	80	68.7	100.0	77.6	98.2
	* 83.2	85	* 76.0	99.5	* 83.2	96.8
	* 88.8	90	* 84.0	99.2	* 88.8	94.1
	* 94.4	95	* 92.0	98.6	* 94.4	91.9
In *water*	*100.0	100	*100.0	97.7	*100.0	88.3

89. In the above, and in the following experiments, the operation of ſucceſſively introducing *moiſture* into the veſſel was ſtopped, when the *ſlip* of *whalebone* was at 80, as beyond that term the ſmalleſt difference in the *temperature* between the parts of the apparatus creates great anomalies; therefore the fol-

lowing *terms* in the three columns of *weights*, which are marked with an *, have only been added (as I have explained for the former *Table*) with the view of having a common *modulum* between the changes of *weight* and the *marches* of the other inſtruments. But the obſerved *terms* remain in their original *proportions*, and from theſe we may ſee, that the *march* of the *ſlip* of *whalebone* does not differ much from the ſucceſſive increaſe of *weight* in its own ſubſtance; and that when taken out of the apparatus, and immediately plunged into *water*, it proceeds in the ſame direction as before, till it has attained its fixed point, while the *hair* takes, comparatively with the increaſe of *weight* of its own ſubſtance, great ſtrides in the beginning of its *march*, and very ſmall ſteps in the latter part of it, before 100; and then *retrogrades* a little, when taken out of the apparatus, and plunged into *water*. We ſee beſides in that TABLE, that the *thread* of *aloes-pitta*, which at firſt takes ſtill greater ſtrides than the *hair*, has, after a longer *ſtationary* or undetermined ſtate, a determined beginning of *retrogradation* at the ſame time that its own ſubſtance continues to acquire *weight* in the apparatus, and continues that *retrograde* march when, being taken out of the veſſel, it is plunged into *water*.

90. The following TABLE will farther illuſtrate theſe characteriſtic differences of *ſlips* and *threads*, the aſcertaining of which was ſo eſſential to *hygrometry*.

III. TABLE *of experiments on the comparative changes in the* weight *and the* length *of the ſame ſubſtance by* increaſe *of* moiſture.

		BOX.		
	Slip of whalebone.	March of the ſlip.	Increaſes of the weight in ſhavings.	March of the thread.
Extr. dryneſs	0	0.0	0.0	92.8
	5	4.5	7.3	87.2
	10	9.5	12.8	93.2
	15	14.5	17.8	97.8
	20	20.0	22.6	100.0
	25	25.7	27.3	95.9
	30	31.5	31.8	92.7
	35	38.0	38.5	88.6
	40	45.5	44.5	79.9
	45	51.5	49.7	70.3
	50	56.5	54.8	63.9
	55	61.2	59.1	57 3
	60	65.7	63.1	51.0
	65	69.7	66.4	47.5
	70	73.7	69.6	40.9
	75	77.7	76.6	31.4
	80	81.5	80.0	21.7
	85	85.9	* 85.0	16.0
	90	90.5	* 90.0	10.4
	95	95.5	* 95.0	5.1
In *Water*	100	100.0	*100.0	0.0

We ſee in this TABLE the *ſlip* of *box* following, in its increaſes of *length*, the increaſe of *weight* in the *ſhavings* of the ſame *wood*, nearly in the ſame manner as the *ſlips* of *whalebone*, *quill*, and *deal*, follow thoſe of their own ſhavings; while the *thread* of *box*, after having gained ſome length by decreaſing ſteps, begins ſoon to *ſhorten*, at the ſame time that its ſubſtance continues to imbibe *water*; being thus the *ſhorteſt*, when it cannot receive any more *water* in its pores. That exceſs of the *hygroſcopic* phænomenon of *threads* cannot but throw a full light on the nature of thoſe *hygroſcopes*.

91. I am now going to aſſemble, in two TABLES, the comparative *marches* of all the *threads*, and of all the *ſlips*, which I have hitherto ſubmitted to that regular courſe of experiments, laying aſide many more of each claſs, the *marches* of which I only know from common obſervations. The next TABLE ſhall contain the experiments on *threads*; in the number of which are two thin natural bodies, which in that reſpect are ſimilar to the *hair*; one, an animal ſubſtance, is a very thin *porcupine quill*; the other, a vegetable, is a thin ſtem of *gramen*.

TABLE *of the correſpondent* marches, *by the ſame* increaſes of moiſture, *of different* THREADS *or vegetable and animal ſubſtances taken* lengthwiſe.

	Porcupine quill.	Whale-bone.	Hair.	Gut.	Aloes-pitta.	Gooſe-quill.	Deal.	Gramen.	Box.	Slip of wh.bone.
Ext. dryneſs	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	72.8	0
	18.0	12.0	15.6	9.7	20.6	37.0	33 2	26.8	87.4	5
	34.0	29.9	29.4	19.2	35.1	66.6	54.8	48.4	93.2	10
	48.8	39.9	40.9	26.8	51.6	78.7	† 74.9	67.1	97.8	15
	62.3	50.8	50.5	37.0	57.6	88.0	84.6	† 76.6	*100.0	20
	73.3	58.8	59.2	47.1	75.6	† 93.4	89.8	83.9	95.9	25
	81.0	65.3	68.8	57.3	71.9	97.2	93.8	90.5	92.7	30
	86.8	70.8	73.0	67.4	76.3	99.0	96.9	95.1	88.6	35
	90.8	76.1	78.3	75.6	83.0	94.4	94.3	98.6	79.9	40
	93.0	81.4	82.1	82.9	† 86.6	96.2	97.7	*100.0	† 70.3	45
	95.0	85.4	86.1	87.8	93.6	99.0	*100.0	98.8	63.9	50
	94.5	88.4	88.8	† 91.6	96.5	95.3	94.6	98.0	57.3	55
	97.0	90.8	91.6	94.7	94.7	97.2	97.0	97.2	51.0	60
	96.5	92.8	93.8	96.3	98.2	98.2	94.6	96.2	45.7	65
	96.5	95.1	95.6	97.8	*100.0	*100.0	93.0	94.8	40.9	70
	95.0	97.1	97.2	98.7	99.2	99.0	91.4	92.6	31.4	75
	97.0	98.1	† 98.0	*100.0	98.2	98.2	89.0	89.8	21.7	80
	98.0	99.1	100.0	98.7	96.8	97.2	86.9	86.5	16.0	85
	98.6	† 99.6	*100.0	96.8	94.1	95.8	84.6	84.0	10.4	90
	99.1	*100.0	99.3	94.5	91.5	94.4	81.9	80.9	5.1	95
In *water*	100.0	99.5	98.3	91.8	88.3	92.5	79.0	77.0	0.0	100

92. There the *porcupine quill* ſhews no *retrogradation*; however, conſiſtent with its tribe, it had ſome in other experiments. Its laſt ſteps have the unſteadineſs of the *ſtationary* ſtate, and thereby are ſubject to anomalies. From the ſame cauſe, none of the other *threads* have exactly the ſame ſteps in any two experiments, though on the whole their *march* remains eſſentially the ſame. The *march* here given of the *hair* hygrometer comparatively with mine, is the mean reſult of three experiments, with three different ſets of inſtruments; one of the *hair* hygrometers that I have employed was ſent to me by Mr. PAUL, of Geneva, and its point of *extreme moiſture* was determined in a *fog*. The ſmall and changeable *retrogradation* of the *thread* of *whalebone* and of *hair* might have been overlooked, were it not for other *threads* in which the *retrogradation* begins before that period where the ſtate of *moiſture* is difficult to aſcertain; but from theſe *threads*, that phænomenon is placed in a clear light, which is reflected on the others. I have marked with an * the greateſt *elongation* of each of them, and with a † a point near which their *elongation* begins, and to which they return at laſt. Theſe ſigns will guide the eye in the above TABLE, which ſhews clearly, that no *thread* can be truſted to for the HYGROMETER.

TABLE *of the correſpondent* marches *of* SLIPS, *or of fibrous vegetable and animal ſubſtances taken acroſs the fibres, and of ſuch as have no ſenſible fibres.*

	Gooſe quill.	Porcupine quill.	Slip of wh.bone.	Box.	Deal.	Ivory. breadthwiſe.	Ivory lengthwiſe.	Tortoiſe-ſhell.	Horn breadthwiſe.	Horn lengthwiſe.
Ext. dryneſs	0.0	0.0	0	0.0	0.0	0.0	0.0	0.0	0.0	0 0
	4.8	4.8	5	4.5	5.4	6.1	8.3	11.0	9.5	13 8
	9.7	8.8	10	9.5	11.2	12.7	16.6	21.5	18.5	26.8
	14.4	12.0	15	14.5	16.5	18.7	24.6	31.5	27.5	38.8
	19.2	17.0	20	20.0	21.9	24.9	31.5	38.5	37.0	48.8
	23.9	23.4	25	25.7	27.2	30.4	37.6	45.4	46.5	58.0
	28.5	29.4	30	31.5	32.7	35.4	43,6	51.9	54.5	64.6
	33.3	36.0	35	38,0	38.3	41.9	49.7	58.3	62.1	71.0
	38.3	41.4	40	45.5	43.7	47.4	56.3	63.8	68.7	75.5
	42.9	45.4	45	51.4	49.2	53.5	62.4	69.0	72.3	78 5
	47.4	49.8	50	56 5	54.6	58.5	67.4	72.8	77.0	82.2
	52.4	54.8	55	61.2	59.9	63.5	71.6	76.4	79.7	86.2
	56.9	59.7	60	65.7	64.9	68.0	76.1	79.4	84.3	89.6
	61.9	64.4	65	69.7	69.7	72.1	79.1	82.4	86.4	92.4
	67.2	68.5	70	73.7	74.5	76.1	82.9	84.9	88.4	93.4
	72.2	73.5	75	77.7	79.0	80.1	86.7	88.2	90.2	94.4
	77.8	78.9	80	81.5	83.5	84.5	90.4	91.2	92.0	95.4
	82.8	83.9	85	85.9	87.5	87.8	92 4	93 8	94.0	96.6
	88.2	88.9	90	90.5	92.0	92.0	94.5	96.2	96.1	97.8
	94.0	94.4	95	95.3	96.0	96.0	97.5	98.6	98.1	99.0
In *water*	100.0	100.0	100	100.0	100.0	100.0	100.0	100.0	100.0	100,0

93. This laſt TABLE is the moſt important, as it contains a claſs of *hygroſcopes* which poſſeſs in common the following firſt requiſites for an *hygrometer*; 1ſt, that they may indicate, without any illuſion, both *extreme dryneſs* and *extreme moiſture*; 2dly, that they move conſtantly in the ſame direction as *moiſture* itſelf; 3dly, that they move always when *moiſture* changes. It ſhould ſeem as if the *march* of the *ſlip* of *horn* taken *lengthwiſe*, from its very decreaſing progreſſion, came very near that of the thin *porcupine quill*; but, as I have ſaid, among the ſteps of the latter there are accidental *retrogradations*, and it ſometimes has a final one; and I have never obſerved that diſpoſition in the former, which, in its laſt ſmall ſteps, follows conſtantly the motions of every other *ſlip*.

94. The agreement of all the *ſlips* in this laſt reſpect is a very eſſential circumſtance in hygrometry, as it aſſures us, that we cannot miſtake the caſes when *moiſture* is *extreme* in the *atmoſphere*; a very important point for diſcovering the nature of many *meteorological* phænomena. No *ſlip* will create deception in that reſpect; while, on the contrary, every *thread* may deceive in dubious caſes, and even create great error, if, unknown to the obſerver, it happened to be in the beginning of its *elongation*. There was, however, a queſtion to be decided in that reſpect, namely, whether or not a great *moiſture* in the *medium* was a cauſe of alteration in the *march* of any *hygroſcope*, by producing in its ſubſtance a ſudden irregular *lengthening*. That accidental queſtion is anſwered in the negative by all the *hygroſcopes* of both claſſes: for, in reſpect of the *threads*, inſtead of *lengthening* ſuddenly in that period of *moiſture*, they have then a *retrograde* motion, either continuing or only beginning; and as for the *ſlips*, they, by *lengthening* in the ſame period, only follow their former *laws*: the *ſlips*

which, comparatively to that of *whalebone*, have at firſt ſmall *ſteps*, and which conſequently move in an increaſing progreſſion, continue only to follow that progreſſion; and thoſe which at firſt have greater *ſteps*, and conſequently a decreaſing march, have then ſmall *ſteps* conformable to their individual *law*; therefore, none of thoſe *hygroſcopes* of both claſſes have any *ſudden ſtart*, produced by any degree of *moiſture* in the *medium*, or by the application of concrete *water*; each of them follows, from one end to the other of its ſcale, it own *progreſſion*; and in reſpect of *ſlips*, moiſture is never *extreme* in the ambient *medium*, as long as, in their reſpective *progreſſions*, they have not attained their *greateſt length*.

95. Our common *hygrometer* muſt then be made of one of the *ſlips*; but with that great diſſimilarity obſerved in their *marches*, which of them ſhall we chooſe as indicating the real *march* of *moiſture*? None as yet from that conſideration, which I do not even think a primary one. It is true, that if we truſt to the increaſes of *weight* in thoſe ſubſtances, as being a means of aſcertaining the real progreſs of *moiſture* in the ambient *medium*, the mean rate of ſix experiments of that kind related in this Paper, give the preference to the *ſlip* of *whalebone*; but this I do not yet conſider as deciſive, farther than in what relates to the comparative *marches* of *ſlips* and *threads*; however, as my reaſons of doubt on a more abſolute concluſion cannot be expreſſed in a tranſitory manner, I muſt lay them aſide for the preſent.

96. But, as I have ſaid before, this is not what ought to determine our choice on the *ſubſtance* of a common *hygrometer*, ſince the obſervations themſelves are diſtinct from the conſequences to be drawn from them. Let us ſuppoſe the caſe (which I do not give up) that, with time and reſearches, ſome

proceſs be found by which *known* quantities of *moiſture* may be ſucceſſively produced in the *medium* itſelf. The uſe of that proceſs for hygrometry will be, as M. DE SAUSSURE has begun to do it, to obſerve, on ſome *hygrometer*, the ſucceſſive effects of thoſe known quantities of *moiſture*, from which may be formed a *table* of the correſpondence between the *equal degrees* of the ſcale of the choſen inſtrument, and the real quantities of *moiſture* in the *medium*; and that *table* will ſerve to correct as well *paſt* as *future* obſervations made with that inſtrument. Therefore it matters not what that *hygrometer* ſhall be, provided it is convenient in other reſpects. Let us then examine which of the *ſlips* poſſeſſes the moſt eſſential properties of an *hygrometer*, ſuch as ſhould be in common uſe for comparative obſervations, and to which conſequently future diſcoveries in reſpect of the real proportions between the quantities of *moiſture* itſelf would be applied.

97. *Steadineſs* is ſurely a firſt requiſite for ſuch an inſtrument; and in that reſpect no *ſlip* comes in competition with that of *whalebone*. That property was the firſt motive of my choice; and as an inſtance of it I ſhall only mention, that I have juſt now plunged into *water* an inſtrument of that ſort, of above ten years ſtanding, which is come to its point of *extreme moiſture* as if it had been fixed yeſterday; for, without regard to the diſtance of obſervations, there may be between them a difference of ſome *tenths* of a *degree*. Some other *ſlips* may be brought to a certain degree of *ſteadineſs* by ſtudying what is the degree of *ſtretch* which they may bear; but that attention is not neceſſary for the *ſlip* of *whalebone*: if, for inſtance, when its point of *extreme moiſture* has been fixed while it was *ſtretched* to a certain degree, that *ſtretch* is

much increaſed, it will acquire ſome abſolute *length*; but it will be *ſteady* again for a new point taken then in *water*.

98. Another property of the *ſlip* of *whalebone*, which at firſt ſhould ſeem contradictory to the former, is its great *expanſibility*, in which alſo it ſurpaſſes all the ſubſtances which I have tried. Such a *ſlip* lengthens above *one-eighth* of itſelf from *extreme dryneſs* to *extreme moiſture*, which produces many advantages in the conſtruction and obſervation of that inſtrument. In reſpect to obſervation, when it is expoſed to the wind, the difference between the chords of the arches of its bends and its real *length* is ſo ſmall, comparatively with its *hygroſcopic* variations, that the indetermination of its *index* will remain confined in a ſpace of one or two *degrees*, when it becomes impoſſible to obſerve *hygrometers* whoſe ſubſtance has but little *expanſion*. Laſtly, of all the ſubſtances which I have reduced to *ſlips*, none is ſo eaſily made thin and narrow as *whalebone*. I have found means for producing eaſily ſuch *ſlips* of it as, with a length of eight *inches*, weigh only about $\frac{1}{6}$th of a *grain*, and are thereby as *quick* as is convenient in other reſpects. All thoſe diſtinctive properties of the *ſlip* of *whalebone* ſeem to point out an *hygroſcopic* ſubſtance fit for our *common hygrometer*.

Deſcription of the whalebone hygrometer.

99. I have now only to deſcribe the conſtruction of that inſtrument as I have fixed it after a long experience. The *fig.* 1. (Tab. IX.) ſhews its form for common uſe. Some of thoſe inſtruments are of the ſame ſize as the *figure*, and they may eaſily be made ſmaller, but commonly they are half as large again in every dimenſion. Their frame will ſufficiently be known from the *figure*,

therefore

therefore I ſhall confine myſelf to the deſcription of ſome particulars. The *ſlip* of *whalebone* is repreſented by *a*, *b*; and at its end *a* is ſeen a ſort of *pincers*, made only of a flattened bent wire, tapering in the part that holds the *ſlip*, and preſſed by a ſliding ring. The end *b* is fixed to a moveable bar *c*, which is moved by a ſcrew for adjuſting at firſt the *index*. The end *a* of the *ſlip* is hooked to a thin braſs wire; to the other end of which is alſo hooked a very thin ſilver gilt *lamina*, that has at that end *pincers* ſimilar to thoſe of the *ſlip*, and which is fixed by the other end to the *axis* by a pin in a proper hole. The *ſpring d*, by which the *ſlip* is ſtretched, is made of ſilver gilt wire; it acts on the *ſlip* as a *weight* of about 12 grains, and with this advantage over a *weight* (beſides the avoiding ſome other inconveniencies of this) that, in proportion as the *ſlip* is weakened, in its lengthening by the penetration of moiſture, the *ſpring*, by unbending at the ſame time, loſes a part of its power. The *axis* has very ſmall *pivots*, the *ſhoulders* of which are prevented from coming againſt the frame, by their ends being confined, though freely, between the flat bearing of the heads of two *ſcrews*, the front one of which is ſeen near *f*. The ſection of that *axis*, of the ſize that belongs to a *ſlip* of about 8 inches, is repreſented in *fig*. 2.; the *ſlip* acts on the diameter *a*, *a*, and the *ſpring* on the ſmaller diameter *b*, *b*.

100. Another conſtruction of the ſame inſtrument is repreſented, of half its dimenſions, by *fig*. 3. (Tab. X.) The eſſential parts of that *hygrometer* are the ſame as in the other; therefore I ſhall only mention the differences adapted to the uſe of fixing it out of a window. The *figure* repreſents the manner in which it is fixed, with its *dial* turned half-way towards the obſerver, by a proper head of the hooks which hold it at the top and the

bottom. The ſteady part of the inſtrument is repreſented by *fig.* 4.; it is partly an half tube, cut longitudinally through the axis, and having ſidewiſe two vertical rows of large holes. A whole tube goes over that half one; for which purpoſe the piece *a*, *a*, is taken off by unſcrewing the female ſcrew *b* which belongs to the part *d*; this laſt is a ſmall open tube, cut outſide in ſcrew. The large external tube has alſo two vertical rows of holes, at ſuch a diſtance from one another, that, when one of them correſponds to one of the rows of the half tube, the other is in front. The two oppoſite poſitions that tube can be brought to are in order that the inſtrument may be placed either ſide of a window, and each of thoſe poſitions is determined by one end of a cut at the bottom of the tube in *a*, *fig.* 3. which then holds againſt the ſteady pin *c*, *fig.* 4. The rows of holes of the tube are to be turned towards the room, to prevent the rain from falling on the *ſlip*; and the *dial* being incloſed in a box with a glaſs in front, no rain can get into the inſtrument. It muſt alſo be fixed in a place not much expoſed to the ſun, or be ſcreened from it without preventing the circulation of the air. The communication of the *ſlip* with the external air through the rows of holes and the open bottom *d*, *fig.* 4. is ſufficient for that claſs of obſervations. By the manner of its being hooked, it may be eaſily taken off for carrying elſewhere; and, if a quick obſervation is wanted, the tube alſo may eaſily be taken off.

I have the honour of preſenting one of thoſe inſtruments to the Royal Society; and, as it is very deſirable that ſome hygrometer be added to the other meteorological inſtruments uſually obſerved, I wiſh this may deſerve a place in their Obſervatory for that purpoſe.

⁂

Fig. 1.

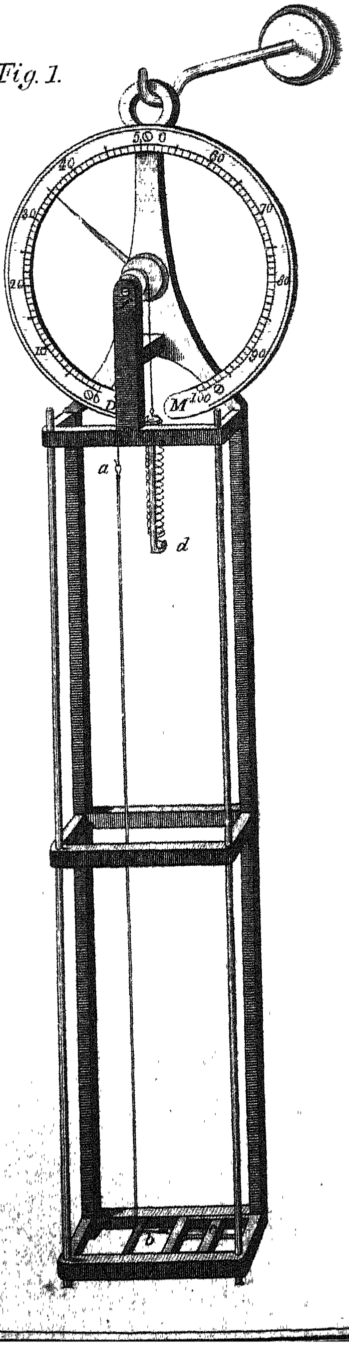

Fig. 2.

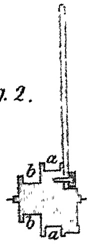

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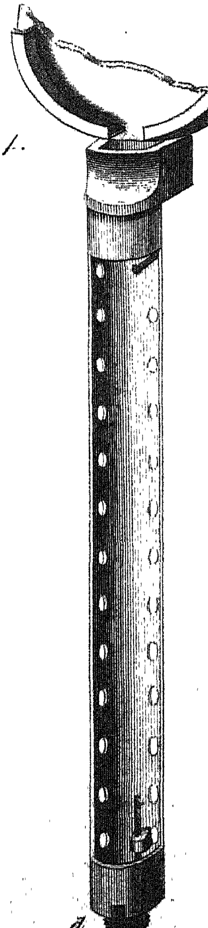
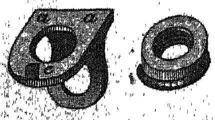
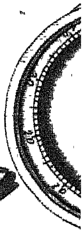
Fig. 1.







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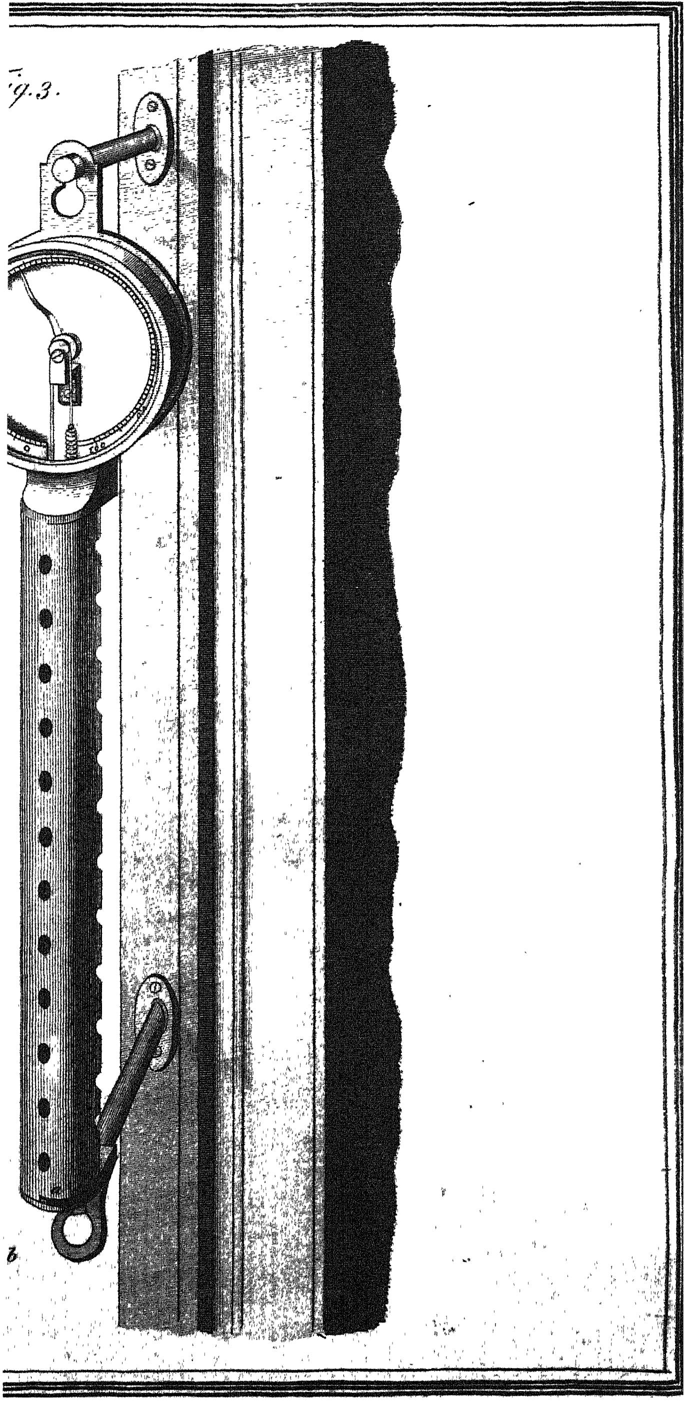

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